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The British Journal for the Philosophy of Science

VOLUME XII MAY, 1961 No. 45

NADEL ON THE AIMS AND METHODS OF SOCIAL ANTHROPOLOGY*

I. C. JARVIE

> Nearly every sociological thesis proposes a new method which, however, its author is careful not to apply, so that sociology is the science with the greatest number of methods and the least results.
>
> Poincaré

1 *Introduction*

IN this paper I discuss critically the late Professor Nadel's views on the aims and methods of social anthropology. These are to be found in his long and difficult book *The Foundations of Social Anthropology*.[1]

Only two problems raised by Nadel will be discussed. (1) What are the aims of social anthropology? (2) What, in general, will a characteristic attempt to achieve these aims look like?

Nadel's answer to (1) is that social anthropology aims at: (*a*) describing and (*b*) explaining rational behaviour. He argues that there is such a close relationship between these two aims that they can be treated as one in the solution to (2), as follows. An explanation is to be effected by means of general descriptive laws, which are 'fit' or 'required' correlations reached by induction. He then argues that we explain the social behaviour of *individuals* by means of laws on non-social (i.e. psychological) levels. We explain social *institutions* in terms of laws specifying the ultimate purpose they serve in society.

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* Received 19.ii.60. This paper is part of a thesis on the methodology of social anthropology which I am engaged in writing. Sections of an earlier draft of it were read to Professor K. R. Popper's seminar at the London School of Economics and Political Science in May 1959; it has benefited greatly from the discussion there. In addition I wish to thank the following people who read and discussed the MSS with me: Dr J. Agassi, Dr I. Lakatos, Mr D. Scheinfeld, Dr P. Stirling, and Mr J. W. N. Watkins.

[1] S. F. Nadel, *The Foundations of Social Anthropology*, London, 1951

Against this powerful position I shall try to maintain that the aim (and actual practice) of social anthropology, like that of any other science—natural or social—is explanatory, i.e. the solving of problems. In criticism of him it will be argued: first, that description and explanation are distinct, the former being a means, not an end; second, that individual social behaviour cannot be explained on any level other than the sociological level; third, that his characterisation of social laws is inadequate for explanation; and fourth, that the functional explanation he expounds explains too much.

Naturally both the criticisms made and the way they are made will be influenced by my own position, which is grounded in Popper's methodology of science. This need not put off those of other views; I only hope the paper provokes them to criticise its Popperian foundations.

2 *Exposition*

2.1 *Nadel's Problem Situation.* What Nadel seems to be doing in the *Foundations* is justifying his claim that social anthropology is a major social science. This is evident when, after declaring that he is writing about *method*, Nadel notes that in earlier days he might have titled his book: ' Prolegomena to the Study of Society: Being an Enquiry into the Nature of Sociological Knowledge '. Social Anthropology, then, is the Study of Society; and to enquire into the aims and methods of social anthropology is to enquire into the Nature of Sociological Knowledge itself. This reflects the view that social anthropology is an all-embracing social science. Social anthropologists often seem tacitly to imply that social anthropology legitimately subsumes under it: sociology, economics, political science, comparative religion, comparative jurisprudence and (in Nadel's case) psychology.

The non-anthropologist might phrase his puzzlement at these grand claims in the form of three questions about social anthrolology (of which the first two are a breakdown of problem (1) of the second paragraph, while the third is equivalent to problem (2)): just what is it about; what does it aim to do; and how does it propose to do it? The answers to these questions will allow us to judge whether, in its aims and methods, social anthropology approximates to our conception of a science.

Nadel admits, a little ruefully, that works on the aims and methods of the social sciences are, if anything, already too plentiful. He cites the brutal comment of Poincaré which heads this paper, and is careful to show that his book on method stems from the treatment of a genuine problem, namely a certain 'lack of agreement' between what is said about method and what the method actually is, in social anthropology.

It can be argued that this lack of agreement arose during the historical development of social anthropology. Anthropology began when, soon after their discovery, people asked why contemporaneous native societies were so different from ours. The original hypothesis was: native societies are different from ours because they are more primitive; they are at a stage our own society passed through long ago. Here an auxiliary hypothesis to the effect that human societies go through a Darwinian evolution from 'primitive' to 'civilised' was introduced.

The original hypothesis could not really be falsified but, instead of looking for ways to test it, later anthropologists made these criticisms: (*a*) such a hypothesis was not gained by scientific methods—careful observation, systematic generalisation, etc.; (*b*) no formulation of it explains all the known facts; (*c*) the problem is uninteresting anyway.

As a result there were modifications; a multi-linear model of the evolution of societies was substituted for the unilinear one. That so little was explained, even after this, was put down to (*a*); (*a*) in turn suggested that the problem was not well put. Interest then turned to a new problem: How do societies so very different from ours, so seemingly irrational, manage to function? Scientific methods were to be used on this problem. Particular cases were successfully explained, but nothing like a general or theoretical science resulted.

Nadel, a Baconian, blames this lack of results not on the methods but on failure properly to carry out the methods. The reasons for this being the intrinsic difficulty of the methods, and anthropologists' ignorance of them. Anthropologists are too wrapped-up in empirical problems to pay much attention to carefully analysing their aims and methods. Thus their (often polemical) methodological pronouncements, which are unavoidable in teaching, bear too little relation to what they are actually doing. There is a lack of agreement between the method anthropologists pay lip-service to, and what they actually *do*; that is to say, between how they *are* going about things and how they *say* they are going about things. He gives no examples, but a famous one is Radcliffe-Brown's brilliant preface to *African Systems* *of Kinship and Marriage* where he strenuously denounces 'pseudo-historical conjecture', and later himself proposes some quasi-historical theories.

This sort of 'lack of agreement' is not an unusual state of affairs in science. Many working scientists are not at all good at describing their aims and methods. Nadel is interested in determining the extent of this 'lack of agreement' in social anthropology. At the same time he hopes this investigation will help to improve matters by 'bringing out into the open what other anthropologists have left unexpressed, rendering tacit methods explicit, and exhibiting their full import' (p. v). Nadel does not say *how* this process will help, but he implies that bringing out these tacit methods will facilitate criticism of them and perhaps lead thereby to improvement.

Nadel justifies his discussion of method on other grounds. He says that he is undertaking a systematic re-examination of 'things known' in social anthropology in the belief that this is a process upon which 'scientific progress rests in large measure'. From this it can be inferred that Nadel felt there was a certain lack of progress, a certain stagnation in the science of social anthropology. He rightly traces the cause of this to methodological shortcomings, and in particular to the problems of observation, description, classification, and explanation. About the first of these nothing will be said. The third—classification—so far as it is interesting, I take to be a part of description. The remaining two topics—explanation and description—are what will be discussed from now on; particularly the former. For I claim it can be shown that Nadel's examination of what is accepted as explanation in social anthropology, his incorrect analysis of it, and his consequent doubts about its adequacy, lead him to his questionable doctrine that there are (non-social) 'levels of explanation' possible in social science.

2.2 *The Subject Matter and Aims of Social Anthropology.* The aims and methods of social anthropology will be discussed in the form of the three questions posed early in section 2.1. 'What is social anthropology about?, i.e. what is its subject-matter?' 'What is it aimed to do with this subject-matter?' 'How is this aim to be achieved?', i.e. by what methods? Let us now take the first two questions.

Social anthropology, he says, works from a basis of 'statements on observation' towards the aim of describing and explaining social facts. But what, it will be asked, are these facts which are to be described and explained? They are, briefly, action and interaction between individuals; 'activity' here meaning aim-controlled or goal-oriented behaviour; that is, rational behaviour.[1]

Nadel's designation of the aims of social anthropology as description and explanation neatly parallels a division of the subject into (descriptive) ethnography and (explanatory) comparative sociology, first made by Radcliffe-Brown. The two 'are facts and theory, a multitude of statements on particular observations and their explanatory synthesis' (p. 21). Nadel thinks this a little misleading, though, since it is undoubtedly the case that ethnography, even in the way its descriptions are organised, embodies (explanatory) theories. Thus description tends to merge into explanation, and for social anthropological purposes 'we may simply equate the two' (p. 21). On a first analysis, then, even that part of social anthropology known as (descriptive) ethnography contains (explanatory) 'theoretical insight of a general nature'; this strengthens the view that social anthropology qualifies as 'a science only to the extent to which it can explain' (p. 20).

From the observed ethnographic fact the social anthropologist aims to synthesise theories. These

> theories are constantly being tested against new facts, often redefined and sometimes abandoned. There are, of course, good theories and bad—theories which account or do not account for the range of facts observed at any time. There are, too, T. H. Huxley's 'beautiful' theories, tragically murdered by an 'ugly little fact'. But the best and most beautiful theory can only account for what is known or observable by existing techniques; and even theories subsequently proved inadequate have added to knowledge or posed problems from which wider knowledge eventually sprang. Thus theories change with observed facts, and observation of facts changes into, and with, theories (p. 22).

One consequence of this attitude to theory is that Nadel attacks the fashionable 'fact worship' among social scientists. By this he means the prevalent empiricist view that what little hope there is for 'scientific' social studies depends on making a determined effort to ground them in a firm basis of brute facts, without any 'premature' attempts to build theories.

Nadel holds that this attitude is fruitless because even '"pure" observation and description' involves 'theoretical insight of a general nature' (p. 11) as we have already seen. This need not imply that

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[1] The notions reflect the influence of Weber and Parsons. It can be seen that if the behaviour is 'irrational' in the sense that it is not aim-controlled then it is sociologically 'meaningless', and presumably cannot be socially explained.

Nadel does not himself believe in brute facts. He adds that 'the intrusion of theory into factual observation can be disregarded by all save philosophers' (p. 24); so that the scientist is free to describe and explain these facts. Nadel seems to mean something like this: on the whole the theories which intrude into factual observation are low-level and generally accepted; but nevertheless, if a philosopher were to examine our statements meticulously he would discover (trivial, low-level) theories embedded there.

2.3 *The Methods of Social Anthropology.* Social anthropology aims to describe and explain rational behaviour. The third question was: 'How is this aim to be achieved?' The rest of this part of the paper will be devoted to it.

Explanation, according to Nadel, is a commonsense concept, slightly refined for scientific purposes, but still basically the same (p. 196). 'Anthropology is a science insofar as it explains', he says (p. 191), quoting with approval Carnap's view (*c.* 1934) that a scientific explanation 'consists in deducing (a statement) from the law of the same form as physical laws, i.e., from a general formula for inferring statements of the kind specified.'

Nadel then argues that, when we explain A in terms of D (but not B and C) we postulate an invariant causal relation between A and D; i.e. we formulate a law describing the connection of A with D. Thus an explanation of A consists in describing its connection with D. Nadel generalises this, saying that explanation is 'nothing but complete description' (p. 199).

But closely as these two concepts of explanation and description are related, Nadel insists that there is, nevertheless, a perceptible step from the one to the other. This step, when pinned down more explicitly, seems to consist in the following two things.

First, Nadel follows Mach in holding that it is a characteristic of *scientific* description that it adds an economical 'plus' to ordinary exhaustive description by '"simplifying, schematizing, idealizing . . . the facts"—facts which can "never be perfectly found in reality"' (p. 202).[1]

And secondly, for any descriptive law to qualify as an explanatory law there must be some 'fitness' or 'requiredness' in the observed regularities described by the law. That is to say, we find it insufficient to be told that two bodies, A and D, always move together, period.

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[1] The quote within the quote is from Mach, *Erkenntnis und Irrtum*, Leipzig, 1920, p. 455.

Because then we always want to ask the causal question: ' Ah yes; but *why* do they move together? ' And to answer this we must postulate some such ' fitness ' as gravitational attraction. It is perhaps more conventional to call gravity an *explanatory theory*, from which a descriptive law is deduced regarding the effect bodies have on one another. However, little depends on words, and it is clear what Nadel means even if his terminology is unfamiliar.

There are, for Nadel, three levels of what I have re-christened explanatory theories. (i) Theories about the whole of society (his ' social concepts ' listed in the next section); (ii) theories about social institutions (functionalism); (iii) theories about individual behaviour for which we must regress to the psychological level. I deal first with (i) and (iii), leaving functionalism a section to itself.

2.4 *Levels of Explanation.* This doctrine of explanatory theories needs to be further expanded. Nadel argues that as every social action is a complex of the psychological, physiological, chemical, and physical processes of individuals it must, ultimately, be explicable in these terms. He describes a hierarchy of the sciences similar to that of Comte (although not credited to him). Nadel holds that mental and physical phenomena, since they constitute the actual level of human life processes, which are necessary conditions of social life, are in some sense ontologically prior to social phenomena. The latter are really ' emergents ' from the neural and physiological levels. True, the picture is complicated by the admitted fact that, to some extent, sociological and psychological phenomena interact with each other.

Nadel now applies this theory of emergents to the problem of trying to explain how one social event actually *causes* another. The result is a further theory, his *theory of levels of explanation*; ' one range of problems posed by social enquiry, namely that of finding a mechanical-causal " requiredness " or " fitness " in the social phenomena, can in fact be illuminated by a move to other, lower levels of analysis . . .' (p. 219).

What this amounts to is that Nadel wants to explain how a social action by one individual causes a social reaction by another. To do this he finds it necessary to assume some internal process in the individual whereby an external stimulus evinces a response in the mind of the recipient which is modified and turned into some sort of bodily reaction. He outlines his conception of these internal processes in terms of neural-physiological theories of ' mental energy ' and ' action potentials '. This is not the place to discuss them.

That Nadel is not, strictly speaking, a reductionist is evidenced by his belief that there are *levels* of explanation. A reductionist holds that *all* human social behaviour can be reduced to, and is best explained in terms of, ' laws of human nature ', i.e. psychology. But Nadel says that institutions and societies cannot be explained in this way. Leaving aside institutions until the next section, we can note Nadel's thesis that as regards whole societies sociologists have developed certain explanatory concepts. He gives as examples of these what might be called ' societal ' concepts the following: ' social integration ', ' mechanical and organic solidarity ', ' social adjustment ', ' optimum size and scatter ', ' cultural adhesions ', ' differentiation in social evolution ', and so on (p. 204). Nadel is not, though, particularly clear about whether the entities treated by these concepts are subject to what he calls the ' phenomenal regression ' of social studies. By ' phenomenal regression ' he means that the subject matter—the phenomena—of social studies ' disappears ' when investigation moves to a lower level (p. 212).

Thus there is no necessary contradiction between Nadel's acceptance of holistic societal concepts on the one hand and his argument that social behaviour can be explained in terms of the lower level human processes it emerges from on the other. For his position seems to be something like this. Holistic societal concepts, the whole idea of ' things social ', are simply useful explanatory ideas, perhaps useful shorthand. But it must be realised that what we call ' social phenomena ' are only *interpreted* as social, for they are really second-order emergents from an interaction of individual neural and physiological processes which is too complex to be specified in full. And these processes are ontologically prior and, perhaps, more ontologically real; and in certain circumstances we interpret complexes of these first-order processes as institutional or societal wholes, to suit our convenience.

2.5 *Functional Explanation.* Human individuals are not Nadel's ' social atoms ' for they are reducible to the various rôles each ' social person ' plays in the society. Each social person is a member of various groups (each of which corresponds to a rôle, e.g. married men, hunters), and these groups are ' actuated ' through social institutions (marriages, hunts), which are thus defined as ' standardised modes of co-activity ' (p. 108). Explaining these standardised modes of co-activity and the interrelations between them is commonly thought to be one of the major tasks of social anthropology. Consequently Nadel's outline and defence of the ' functional ' theory of social institutions is of great interest.

We have already seen that, in Nadel's rather Machian view, science is simply (economical) description of the invariant relations between facts, made intelligible by being subsumed under some ' required ' explanatory theory. The explanatory theory under which ' functional ' relationships between social institutions are discussed is a much debated matter. Nadel distinguishes at least four distinct ' functional ' theories[1] about the relations between social institutions. I will outline the four theories with the help of my own examples.

(I) The first theory is the common-sense one that the function of a social institution is the job that it performs; e.g. ' Among some remote Bedouin the feud still functions as a form of redress in cases of homicide '.

(II) The second theory is a strengthened version of the first to the effect that *every* institution of a culture has a job of work to do in that culture; that no part of the culture is ' random ' or accidental or a functionless survival. This theory would tend to take the form of methodological prescriptions; e.g. ' Perhaps it is hard to see what the function of the feud is among the Sicilians now, but there must be *some* reason for its continuance—look closer, try again! '

(III) The third theory is a weakened version of (II) which states that even if every part of a culture has no specifiable job, at least most parts of the culture influence and mould other parts; the parts are interdependent. The degree to which they are interdependent in any particular culture is a matter for investigation. An example of this quasi-mathematical theory in practice would be the statement: ' It has been found that in most of Africa the strength of the institution of the feud is an inverse function of the strength of a centralised judiciary '.

(IV) The fourth theory is an alternative version of (II) to the effect that there is some sort of ulterior (but empirically appropriate) purpose built into all social institutions. This is an interesting example of a metaphysical (or, more precisely, unfalsifiable) theory with, perhaps, valuable methodological consequences; because it urges the anthropologist to seek out such a purpose in all cases, even when, as in the following example, it leads to counter-intuitive results: ' Superficially the feud seems to be a mechanism for vengeance in cases of homicide, but the overt hostility of the parties conceals a link binding them together. For if homicide by a member of one group against a member of another provokes only a feud, as opposed to a war, this shows that the groups are less hostile than they would have been if they *had* gone to war. And if a third group should attack either of the two groups in a feuding relationship, the latter will band together to make war on the third group. Thus the function of the feud is to promote a kind of cohesion between groups.'

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[1] He actually conducts his discussion in terms of the meanings of the word ' function ' (pp. 368-369). I think my presentation is clearer, and facilitates criticism. (Theory IV is a version of the theory developed by H. M. Gluckman in *Custom and Conflict in Africa*, Oxford, 1955.)

As these theories are not all compatible with each other some have to be ruled out. Theory (I) can be eliminated[1] as trivial and obvious, theory (II) as obviously false, its useful parts being present in (III) which, in turn, according to Nadel, merges into (IV). (We might add that (III) begs the question of 'fitness' or 'requiredness' altogether, as it postulates a quasi-mathematical correlation, and quite unconnected things can often be correlated.) This leaves him with theory (IV) to which he adds the auxiliary hypothesis that the social institutions of any relatively stable society tend to become more harmoniously integrated. (Further reference to this last assumption will be made in the next section.) An example given by Nadel of this pragmatic theory (IV) is the case of the first fruits ceremony. He points out that as well as all the professed reasons for

> permitting the harvesting and consumption of first fruits only after certain sacrifices or rituals have been performed . . . we can substitute an empirical appropriateness for the . . . mystic one. For we should argue that the taboos in question enable men with expert knowledge to control these important activities, so that labour will be efficiently co-ordinated, the premature dissipation of food stocks prevented, and perhaps also the danger to health obviated which might be caused by eating unripe crops (p. 274).

Against the suggestion that the above was just *post hoc* rationalisation, Nadel could have argued that this constituted a genuine explanation because it shows how the social institutions of this culture unwittingly take care of certain matters crucial to the welfare of that culture. He could have shown that when we speak of 'function' in the sense of theory (IV) we are in some way specifying a necessary condition for the survival of the culture. And if the notion that there are specifiable necessary conditions for the survival of cultures is combined with the minor premiss that this culture has survived then we can legitimately infer that it must have handled these necessary matters somehow. A culture, then, to take the argument one stage further, has the (unintended?) purpose of bringing about the survival of itself. That is, those cultures which survive do so by taking care of certain vital matters better than others. Once we have accepted the simple notion of societies taking part in a Darwinian evolutionary struggle for survival in adverse conditions, we can start speaking in terms of their state of health. Consequently it is possible for Nadel to talk about ' social pathology '.

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[1] Nadel eliminates them on slightly different grounds but, again, besides being stronger, my criticisms make for easier exposition.

3 *Criticism*

3.1 *Background to the Criticisms.* The points made by Nadel which will be criticised are: his conclusion that the aims of social anthropology are: (*a*) explanation and (*b*) description, but that the former can be reduced to the latter; his thesis that there are levels of explanation; his interpretation of the laws of social science in an inductivist way, i.e. the view that social laws are factual generalisations which, when they are ' required ' or ' fit ' constitute explanations; his outline of a functional explanation and its teleological implications.[1]

To avoid misunderstanding of these rather negative criticisms, the spirit in which the attacks are launched and the general methodological position from which they are directed should be indicated.

The fieldwork of British social anthropologists is of very high calibre. They go to a strange society and succeed in explaining what often appears to be the most irrational behaviour in terms of the situational logic of that particular social organisation. Their publications, too, are often illuminating and suggestive. Their procedures come into question when they *invert* what Popper calls the *unintended* consequences of some actions; that is, they take the obviously unintended consequence of some action, and proceed to say *per contra* that *the function of that institutionalised action is to bring about those (unintended) consequences* (*vide* Nadel's discussion of the (IV)th theory of function). Doing this gives a perfectly good notion—events have consequences—a dangerously teleological twist. Using ' function ' in this sense implies that the consequences of the event are intended; and that they are intended, and must be intended by someone or something, further implies some kind of supra-individual with motives or purposes or ends which has caused them to come about. This contradicts the metaphysical theory, for which overwhelming arguments have been presented (see the first five items in footnote 3, p. 18) that *human individuals are the only causal factors in society.* From this theory can be derived the axiomatic methodological prescription: 'Explain all social events in terms of human factors.'[1] Metaphysical theories cannot be falsified, of course; but they can be critically discussed as in the rest of this paper.

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[1] I should perhaps add that without, to my knowledge, distorting his *views*, I have sometimes invented new arguments in support of his views, and have systematically eliminated anything he said which seemed to weaken his case.

3.2 *The Subject Matter and Aims of Social Anthropology.* Nadel has taken up the position that the subject matter of social anthropology is rational behaviour. The main aims of social anthropology are to *describe* and *explain* this subject matter. He argues that, in the last analysis, explanation can be reduced to 'economical' or 'fit' description. These aims are to be achieved by the methods of the inductive natural sciences: collecting facts, using these to eliminate dubious hunches, then synthesising them into true natural laws.

Against this I shall maintain that the subject matter of social anthropology is problems; that its aim is to solve these problems; and that this aim is best pursued by the rational or critical method of advancing bold hypothetical solutions and doing our best to criticise them.

Earlier it was suggested that social anthropology arose from, and still centres around, the kind of *problems* mentioned in 2.1. Few would dispute this until, perhaps, they see how it implies that the aim of the subject is solving these problems; that is, explanation. Description, it seems, does not come into it. So the important question now is this.

Has Nadel made much of a case for accepting description as an aim, i.e. as an end in itself? Is it more of an end in itself than the formulation of problems as clearly and simply as possible is an end in itself (important though it is)? Is it not rather a means to a further end, namely the attempt to solve the problems? Nadel's elevation of description to an aim, then, now depends entirely on his attempt to reduce explanation to description; and some reservations are in order with regard to this. It is easy to see what he means, of course. When we don't understand something we want to know its cause, and an explanation of *why p* causes *q* will in fact be a description of *how p* causes *q*. The explanation will be a deduction from a set of statements including one describing a law of nature, whether that be an eternal verity laid up in heaven, or simply a fortuitous regularity in the face of cosmological indeterminism. But Nadel himself is a little worried by the success of his reduction so he adds that scientific description is both more economical than ordinary description, and that it must have an element of ' fitness ' or ' requiredness ' if it is to be satisfactory. His own subtlety has caused him to miss the main point here. This is the evident fact that when we have the causal mechanics behind some event described to us, an element of *understanding* is imported; our knowledge of the causal antecedents of the event allows us, provided the ' why ' questions are not carried back too far, to say, to all intents and purposes, why *p* has caused *q*.

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[1] The end of the paragraph is based on J. W. N. Watkins's note 'The Two Theses of Methodological Individualism', this *Journal*, 1959, 9, 319-320

In other words this assimilation of the notion of explanation to description, although in some ways correct, is misleading. Subsuming explanation under the descriptive statements used *in such* an explanation tends to blur the fact that science has an explanatory, as well as a descriptive, function and that these two things are not the same. In this respect Nadel's whole treatment of explanation and description, like many attempts to refine or explicate our ordinary-language concepts, seems rather fruitless. If the common-sense view of explanation is not distinct from description when analysed closely, and given that a distinction seems necessary, all the more reason for sticking strictly to the technical notion of explanation invented by logicians to describe what happens in science. Nadel makes surprisingly little use of Carnap's model of explanation, which presumably was introduced into methodological discussion just because ordinary language was found to be too imprecise. Explanation in this technical sense means the process of deducing a statement describing what is to be explained from a universal statement (together with certain statements of initial conditions). Now this process of *deduction* cannot be reduced to description, even though all the statements in the deduction are descriptive ones. Also, that this technical notion in fact approximates closely to our intuitive idea of what it is to explain something should become clear in the next section.

There is only one other point to be made in this connection. Were description an end in itself it would have to be exact and ' objective ' or ' scientific '. But such description is a scientistic myth; or, in other words, all description is selective and our selection is governed by our interests; that is, the problems we are interested in solving, the theories we are interested in testing. So scientific description *cannot* be the aim of social anthropology.

Thus the only aim left for social anthropology is explanation. Nadel's view that social anthropology is a science has been accepted; together with the view of those philosophers of science who hold that the generalised aim of all scientific activity is to find testable, causal explanations of the kind outlined above. Description plays two main rôles in this. The first is in the formulation, as clearly and simply as possible, of problems. The second is in the marshalling of facts with which (or against which) to test the hypothetical solutions to these problems.

3.3 *The Methods of Social Anthropology: Explanation.* In order to examine Nadel on explanation in detail his own example of the joking relationship, a typical anthropological problem, will be analysed.

> In many primitive societies grandfather and grandchild stand in an especially intimate relationship, which is friendly and of equal footing, and implies that familiarity and privileged disrespect which anthropologists call 'joking relationship'. Between grandfather and grandchild there is none of the disciplinarian attitude and demand for respect which characterises the relationship between a father and his boy and their respective generations. On grounds of general knowledge we suspect where the relevant conditions for this state of affairs may lie. They would seem to lie in the fact that the grandfather stands, by his age, on the borderline of social usefulness and, by his generation, on the border of the effective family group. He does not *need* to exact the respect of the growing generation, while the father, who directs the family and the education of the young, *must* do so. The grandfather-grandchild relationship would therefore seem to offer a *relief* from the sterner atmosphere of authority which otherwise dominates the relationship between the child and the adults of his family (p. 235, italics mine).

The problem is the existence of a 'joking relationship', rather than a relationship of respect, between grandfathers and grandchildren in many primitive societies.

The explanation—'on grounds of general knowledge'—is this. Because the grandfather is not very useful socially and because his age puts him on the outer fringe of the family group, he does not need to exact the respect of the younger generation. In other words, because his responsibilities are low the grandfather can afford to offer a 'relief' from the sterner atmosphere which otherwise pervades in adult-child relationships.

Such a hypothesis is well suited to comparative testing, but before such empirical tests are resorted to it should be critically examined to see whether it is a *satisfactory explanation.*

The explanation has the following form:

(i) LAW STATEMENT: Adults exact from children the amount of respect they need to maintain their position.

(ii*a*) STATEMENTS OF INITIAL CONDITIONS: Grandfathers do not need respect from children
(ii*b*) Grandfathers are adults (analytic).

(iii) CONCLUSION: Therefore grandfathers do not exact respect from grandchildren.[1]

This should make the structure of Nadel's explanation clear.[2] True, the law statement should specify more precisely the limiting conditions of family organisation, political structure, and so on. Nevertheless we see at once that the law statement is problematic. Leaving aside the question of whether respect is not *desired* as well as needed, we can see a mistake in the notion that the respect a child pays to an adult depends solely on the wishes of the adult. After all, respect is given as well as received. Children are not automatons who respond immediately to the wishes of an adult—a child very often does not do what his elders want him to do—both actors, in fact, contribute to the pattern of who respects whom. This argues that the law statement extracted from Nadel's text oversimplifies the story of why certain socially normal patterns of respect exist; and because it ignores some obvious factors intrinsic in respect-relationships; is not a very satisfactory explanation of the problem. Nadel rather slurs over the question of why people conform to this norm; that is, what factors maintain it. The whole matter is difficult, certainly; all the discussion tries to show is that *purely methodological criticism* indicates further thought is needed before empirical testing will be worthwhile. For example, it is not even clear whether an explanation of joking relationships is best sought in terms of the unintended consequences of certain other factors in the situation of the two actors; or in terms of a conscious intention of the actors themselves to bring about the state of affairs which is thought in need of explanation. (Such a conscious intention is *almost* implied by Nadel when he speaks of 'relief' from a posited 'tension' in the authority-relationship normally obtaining between children and adults.)

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[1] Where (i), (ii*a*) and (ii*b*) constitute the *explicans* and (iii) the *explicandum.*

[2] It has been pointed out to me that Nadel's formulation of this argument is not as good as it could have been, and that he was writing in abbreviated form for an audience with specialist knowledge, some of whom have produced better versions. This means that my criticisms of his example cannot be as strong as I should like because although Nadel uses very few examples they were obviously chosen with great care and are not therefore to be lightly modified or 'improved'.

3.4 *Levels of Explanation.* In section 2.4, in order to defend the theory of levels of explanation, Nadel's argument concerning the ontological status of social reality was given. To avoid pursuing the topic in that direction a logically weaker version of the theory will be dealt with to the effect that there are some sociological problems which can (and some which must) be explained on other levels, i.e. the levels of psychology and physiology.

It has already been argued that Nadel is not strictly speaking a reductionist, yet his doctrine of a phenomenal regression seems to come to much the same thing. He argues that some social phenomena 'disappear' when they are described in psychological and physiological language. This 'disappearance' means that the translation from sociological to psychological and physiological language can be carried out *without loss of informative content.* But this kind of translation would be an explanation in itself. Nadel accepts this point when he argues that the model we employ of a 'rational action' itself entails the assumption (i.e. must ultimately be explained in terms) of psychological mechanisms like 'mental energy' and 'action potentials'. Perhaps it does, but does not this beg the question of whether such models of rational actions are sufficient to explain social behaviour? That is, perhaps we can regress to other levels of explanation or description, but if we do, are we answering the same questions, are we tackling the same problems? Clearly not, for when we ask why there is a joking relationship in society x we are not likely to be satisfied with an answer like: 'Because people in x have a psychological disposition to do so'. Because this is the point. What we want to know is what it is in x which causes the people in x to have this psychological disposition, since it does not appear to be a universal human attribute. And since any answer will involve both heredity and environment, and the latter is partly social, it can be seen at once that psychology cannot be used to explain 'things social', for it is in its turn dependent upon them.

And if it is remembered that the problems of the social sciences largely concern the unintended consequences of our actions, it will be clear that psychology is irrelevant. For how can the *repercussions* of our actions, and the unintended repercussions at that, be reduced to psychology or explained on any other ' levels '.

There remains a decisive and possibly unanswerable objection to the whole theory of levels of explanation. The argument rests on the fact that (as Nadel himself points out, although he does not see the implications) there is some interaction between the different levels. That is to say there is interaction between sociological and psychological (and perhaps physiological) phenomena. In other words, granted that social life is to some extent a product of things happening on the psychological and even physiological levels; it must not be forgotten that in turn there is a reverse process whereby things happening on the social level influence psychological (and even physiological) processes. Nadel fails to notice that this interaction contradicts the theory of levels: it contradicts the notion that any one level has priority over any other,[1] and with that the idea that any one level can explain or be reduced to any other. For if psychology and physiology are necessary conditions for sociology, and there is interaction, then sociology becomes a necessary condition for psychology (and perhaps in more complicated cases, physiology; e.g. it is possible to envisage circumstances in which anti-social tendencies might result in a person dying). The levels are thus mutually interdependent and therefore co-present. No event can be satisfactorily explained on any other level than its own. Any attempt to explain, e.g. the social environment in terms of psychology will have to include the social environment which contributed to the psychology, and thus the explanation will be circular. And if psychology is taken to mean neural processes (as Nadel at times seems to) then the social environment can influence neural processes, i.e. physiology, and the circularity of the theory of levels is further reinforced.

The upshot of this analysis is that there are no levels; there are only problems and explanations; and explanations are best judged solely by the criterion of whether or not they provide satisfactory answers *to the questions asked.* The notion of moves to lower levels of explanation is irrelevant here, for what we are interested in is causal explanation. An analysis of the individual's situation is sufficient to explain social behaviour causally. It looks as though what Nadel is really gnawing away at, with his discussion of levels, is the totally different question of mind-body interaction. There are good

[1] Including even the ontological priority not discussed.

reasons for supposing that a solution to this problem is in a certain sense logically impossible.[1] Be that as it may, Nadel has no warrant for the way he blandly assumes monism and then tries to build it into social anthropology. Had he realised what he was doing[2] his argument would doubtless have been different.

Nadel is in many ways a good example of his own criticism in that there is a clash (' lack of agreement ') between what he advocates and what he does; e.g. in handling the joking relationship, as we saw in section 3.3, he uses neither holistic concepts nor psychology, and his explanation does not seem to be functional (this is discussed in 3.6). Now his psychologism has been rejected as circular. If we are also to reject his holism for individualism we must answer the question: is there a methodological individualism which is not psychologistic? This crucial methodological ground has been well trodden by Hayek, Popper, Watkins, Gellner, and now Agassi.[3] It is, however, a possibility which Nadel overlooked, although he came near to it in his joking relationship example. How *would* it work out in terms of the joking relationship?

Presumably a psychological individualist explains this by means of some such notion as ' laws of human nature ' concerning affection and respect, and how these emotions are encouraged in certain directions in some societies and in other directions in others—the joking relationship society being one of the others.

This explanation is already circular, of course, because sociological terms have had to be reintroduced. The trouble is caused by the fact that psychologism cannot account for social institutions (see the authors cited).

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[1] I must thank Mr Watkins for making this point clear to me.

[2] That he did not is surprising in view of the extent of his knowledge of the philosophical literature revealed in the bibliography.

The mind-body problem arises in Nadel's book only because he neglects his own assumption that we are considering rational behaviour (we might, following Popper, call the assumption ' the rationality principle '; see section 2.2, text to footnote 1). Given this assumption we do not need to know anything about the mental processes involved in taking a decision. All we need, to assess its rationality, is the information in the light of which it was taken, given the ends to be attained.

[3] F. A. von Hayek, *The Counter-Revolution of Science*, Glencoe, 1952; K. R. Popper, *The Open Society and Its Enemies* (3rd edn.), London, 1957, ch. 14, and *The Poverty of Historicism*, London, 1957, sections 29, 31, 32; J. W. N. Watkins, ' Ideal Types and Historical Explanation ', this *Journal*, 1952, **3**, 22-43, and ' Historical Explanation in the Social Sciences ', this *Journal*, 1957, **8**, 104-117; E. A. Gellner, ' Explanation in History ', *Artist. Soc. Supp.* 1956, **30**, ' Dreams and Self Knowledge ', 157-176. I want to thank Dr Agassi for letting me read his paper ' Methodological Individualism ' (*Brit. J. of Sociology*, 1960, **11**, 244-70) in MS. I found it very helpful.

A methodological individualist would ask (among other things) what factors in the situation of an anonymous grandfather in this society make him want (i.e. act to bring about) a different relationship with his grandchildren than with his children. The answer will show how the joking relationship is tied in with other features of the society which help maintain it by creating circumstances which make the grandfather decide to act in accord with, rather than against, the traditional norm. (This explanation assumes only the rationality principle assumed by Nadel himself—see footnote 2, p. 18—and that we can know the grandfather's aims and the state of his information.)

3.5 *Induction and Social Laws.* Induction, by which is meant the generalising of laws from collections of particular facts, is an interpretation of the methods of science not accepted here. But it must be discussed because Nadel contradicts himself over it in a rather important way. He maintains induction on the one hand by claiming that theories are somehow synthesised from facts; then on the other hand (p. 224) he in effect gives up induction by introducing a notion of 'pre-conceived ideas', which are similar to Bacon's *anticipatio* ('anticipations' is Nadel's term). These 'anticipations' are 'some preliminary hypothesis or suspicion as to the kind of correlations likely to prove relevant'. But if he has a hypothesis, however preliminary, the deductive consequences of which will presumably be tested against the facts, then he is using the hypothetico-deductive method which is logically incompatible with induction; and so he must abandon the latter.

Underlying the contradiction between the inductivist desire for deductivist certainty, and the necessity of having uncertain preliminary hypotheses, is a serious problem. It is, in fact, Bacon's problem of the tendency all theories have to verify themselves. If we hold false theories our minds will be prejudiced by them and any evidence we collect to test these theories will be seen in their light, and distorted in their favour, like looking through coloured spectacles. The theories will only allow us to see favourable (or confirming) and neutral evidence, rendering us blind to the rest; and this distortion will become so acute as we build up more and more theory-impregnated facts that we will never be able to get rid of the false theories.

According to Bacon this problem can only be solved by ridding the mind of all theoretical 'prejudice' so that it is pure the observation of facts. When the pure mind observes, theories will somehow leap into it. Any theories which come into the pure mind will be *ipso facto* true because they will be derived from unprejudiced observation of Nature, and Nature does not lie.

Nadel's difficulty with this arises, perhaps, because he can see the fruitlessness of the empiric's collection of facts, and yet he is not convinced by the ' pure mind ' solution. So he reintroduces the (tabooed Baconian) idea of ' anticipations '; suspicions or hunches about which facts will be significant. But then these anticipations at once lead him into precisely the verificationist trap that Bacon strove so hard to avoid. So the problem Nadel now has to solve is how to prevent hunches about which facts will be significant from becoming prejudicial; without, that is, going so far as Bacon and systematically eliminating them.

Nadel's solution to the problem is to minimise it. He thinks that we clear our minds of theoretical prejudice by an effort, and that the theories left after this has been made will not be prejudicial. And if this is the case then only a philosopher need attend to such minimal colouring as the mind still suffers from. He seems to think that once we have made *a determined effort* to exorcise the prejudicial effect of theories, and tried to get at really ' pure ' nuggets of hard fact, then we can consign the rest of the task to the philosopher—who deals with details. But just by thinking that the prejudicial effect of theories can to all intents and purposes be eliminated by an effort, he commits the cardinal sin. For Popper has strengthened Bacon's argument to make the point that we can *never* escape from theories; our minds are, perhaps, naturally, inescapably prejudiced. The best we can do is face this situation squarely and do our utmost to make explicit any theories we hold so that they can be criticised. We should never make the mistake of believing that we have reached rock bottom where ' the intrusion of theory into factual observation can be disregarded by all, save philosophers ' (p. 24).

There are several other points concerning Nadel and induction. It is fairly well known that induction can be shown to be both logically impossible and to lead to contradictions. Various philosophers have tried to escape from these criticisms but none have succeeded. Yet if Nadel is going to accept Bacon's solution to the problem he should take care to avoid the very pitfalls Bacon himself pointed out. In fact he rejects Bacon's method of avoiding the verificationist trap and substitutes a solution of his own which plunges him straight back into it.

Another question still remains. Whether or not in the light of the above criticisms Nadel would have been able to retain his view that social laws simply come down to ' fit ' or ' required ' correlations? The view taken here is that in science ' law ' is most usefully confined to physically necessary causal connections between phenomena. Nadel might have countered this by asserting that social laws are different from the laws of natural science precisely in that they describe contingent regularities and not necessities; in which case he would be wrong. Wrong simply because there exist counter-examples: e.g. ' All social changes create vested interests which resist further change '; and many principles of economics. And he could hardly have argued that in social science both contingent and necessary regularities are called laws because the difference between them is not important. I hope this will not be taken as merely a dispute about using the word ' law '. It is actually about whether the aim of science is achieved by means of inductive generalisations, or by strict laws of physical necessity; and whether the two are usefully kept distinct. My position is that the two *must* be kept distinct and that science aims at the latter. This paper is not the place to justify this position but perhaps enough has been said to indicate that Nadel's view is unsatisfactory.

It would seem, then, that Professor Nadel thinks the science of social anthropology proceeds by means of inductive generalisation, a view rejected in this paper. He could have countered this by asking how social anthropology does proceed if it does not use induction.

Let me briefly indicate an alternative view of the logic of the social anthropologists' procedure. Like Nadel I think that social anthropology is a science, and I hold that the aim or task of science is to explain the world. Social anthropologists seem to me to be largely concerned with two categories of problems which I shall call *local* and *general*. Locally they are concerned to explain problematic situations in any particular society. They might, for example, find a society in which all senior males are paid respect, with the sole exception that between grandfathers and grandchildren there is a joking relationship. This is a local problem of explaining conformity to the established norms of behaviour in a given society, in terms of the aims and situation of anonymous individuals. On the general level, however, their problems arise from comparing societies and trying to understand why they differ. Why, e.g. one tribe has a joking relationship between certain relatives, but the neighbouring tribe, although in other respects similar, has not. Or, for example, how law and order is maintained in certain Polynesian societies despite the absence of government machinery similar to that in the West. The former requires a historical explanation; the latter is a problem of the unintended consequences of institutions.

3.6 *Functional Explanation.* As to functionalism. Nadel claims to be most interested in the (IV)th functional theory, namely the one which looks for some ulterior purpose in social institutions. Yet he ignores this theory when he tries to explain the institution of the joking relationship. Perhaps he realised how little real explanatory power there is in the (IV)th theory of function. I suggest, in fact, that he is really interested in theories (II) and (III), i.e. the notions that *all* social institutions have a rôle in society, and that there are discoverable invariant relations between social happenings. The first theory is metaphysical but methodologically useful, the second theory has been fully overhauled in the previous section. In the joking relationship example Nadel seems to neglect functional explanation for the logic of the situation which he does not handle too well. Although, in all fairness, we *could* argue that the grandfather's lack of function, the fact that he has few rights and duties towards his grandchildren, and expects few from them, allows him to have a more friendly, less disciplinarian relationship with them. Yet this explanation cannot be a part of Nadel's (IV)th theory of function, and it presents the following difficulties for theories (II) and (III). What is the joking relationship's sociological rôle? What is the joking relationship correlated with?

The joking relationship example has shown enough and need not be gone into further. We can now see that there is at least one problem which functionalism cannot satisfactorily explain. That is all we need. Functionalism is a very important doctrine and in some forms has *solved* extremely difficult problems. But it does not, by any means, solve all the typical problems of social anthropology,[1] and if it is Nadelian functionalism (theory (IV)) it seems not to solve any problems at all.

3.7 *Purposive Explanation and Teleology.* By and large Nadel does not exaggerate the explanatory power of holism; if by 'holism' we mean speaking metaphorically about institutions as though they were wholes, when describing the situational logic. But severe reservations about its use in other contexts have to be lodged; these are well known and need not be rehearsed here.[1] Instead, I wish to give an illustration of the way holism (of the kind at the back of the (IV)th theory of function), quickly leads to uncritical explanations.

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[1] Cf. Kingsley Davis for the opposite view in 'The Myth of Functional Analysis as a Special Method in Sociology and Anthropology', *American Sociological Review*, 1959, 24, 757-772

> Society and culture are made and worked by man. May we not assume that they are made and worked for man? The Great Engineer is merely Man in the abstract, and the Intelligence at the back of all things social, the Human Mind writ large (p. 368).

As R. Needham has pointed out[2] this disturbingly Whiteheadian passage would be less troubling if we knew what the capitalised entities were. What do these metaphysical conjuncts about the Great Engineer and His Intelligence explain? How do they differ from the belief that the universe reveals a Divine Pattern? It is surprising that people should believe such hypotheses explain anything; the fact is that they explain everything, and that is too much. Any hypothesis which explains everything, gives no causal, *testable* explanations at all.

We come now to the final objection to Nadel's (IV)th theory of function which, by explaining an institution in terms of what purpose it serves, is teleological. And teleological explanations are not the causal explanations in which science is interested. For they are ultimate and final explanations and Popper has argued convincingly that there is no place for ultimate explanations in science;[3] there is always room for better and higher-level hypotheses. Emmet's contention that if functionalism is teleological then it is 'teleological in complicated ways' does not seem to escape the criticism.[4] And weakening the theory to make it less open to criticism also makes it trivial.

4. *Conclusion*

The principal object of this paper has been to outline and criticise Professor Nadel's view of the aims and methods of social anthropology. The interest shown by Nadel and other leading British social anthropologists in such basic problems strikes me as a result of the development of the subject into a major social science.[5] Nadel has thrown light on many difficult and interesting problems. His principal thesis that social anthropology is a science has been accepted, so far as it accords with Popper's view of science; where there is a clash Nadel has been criticised. The point of all this being that social anthropology still has great untapped potential; and, as Nadel argues, it is untapped at least in part because of faulty methodology. My personal conviction that the way out of the impasse for social anthropology, as for the other social sciences, lies by way of methodological individualism has been strengthened by working over Nadel's arguments.

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[1] See K. R. Popper, *The Poverty of Historicism*, London, 1957, sections 23 and 24

[2] R. Needham, in a review of Nadel in *Man*, 1951, 51, 130–132

[3] K. R. Popper, 'The Aim of Science', *Ratio*, 1957, 1, 24–35

[4] Dorothy Emmet, *Function, Purpose and Powers*, London, 1958, p. 7

[5] Methodological preoccupations are either a symptom or a disease—I prefer to regard them as a symptom.

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STATISTICS AND SELECTION *

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I

THE shadow of Archbishop Paley and the argument from design still broods over evolutionary theory. Although the inference is now not to a Contriver but to Natural Selection, the premise from which the argument starts is still the same: the phenomenon of adaptation, the remarkable fashion in which tissues, organs, organ systems of living things appear 'suited to' the functions they perform. This premise is sometimes baldly stated in the identification of animals with machines, sometimes left implicit, as with Darwin, in the direction and emphasis of evolutionary theory, but its rôle is crucial in either case.

What I want to talk about here, however, is not the axiom of adaptivity, as one may call it, in its whole range, but one particular form of evolutionary theory in which the pervasive importance of adaptation has the status neither of an explicit postulate nor of an implicit belief, but of a proven theorem. This is, or seems to be, the case in Sir Ronald Fisher's *Genetical Theory of Natural Selection*,[1] a work which has played a decisive part in the rise of neo-Darwinism or, as it is sometimes called, 'the synthetic theory'. It is the conceptual structure of Fisher's theory that I want to examine. Again, not all theories of evolution, not even all neo-Darwinian theories share this structure; but Fisher's argument has been so very influential that it seems worthwhile to examine it in some detail.[2] I shall take the mathematical core of the theory for granted, but want to examine the concepts carried by the mathematical formalism.

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* Read to the British Society for the Philosophy of Science, May 16th, 1960

[1] Oxford, 1930 (New York, Dover 1959)

[2] It is often said that Fisher has 'proved mathematically' the truth of neo-Darwinism. The confidence with which the 'synthetic' theory has been asserted on the basis of Fisher's argument is reflected, for example, in the contributions by Huxley, Fisher and Mayr in Huxley, Hardy, Ford, *Evolution as a Process*, London, 1954; or in the conclusion of the third edition of de Beer's *Embryos and Ancestors*, Oxford, 1958, or in P. M. Sheppard's *Natural Selection and Heredity*, London, 1959—to mention but a few examples among many.

As Fisher writes, 'the rigour of the demonstration requires that the terms employed should be used strictly as defined'.[1] Yet his argument, as a demonstration of the effectiveness of natural selection throughout the evolutionary process, depends, as I hope to show, on the use of his fundamental concepts, not always 'strictly as defined', but in a number of senses, one 'strict' and several more comprehensive.

2

It is the principal thesis of all Darwinian and neo-Darwinian theories that evolution is the result of the joint action of random variation (or mutation) and environmental pressure (or natural selection). How is this basic thesis supported by Fisher's argument in the *Genetical Theory*? I am not asking whether evolution is in fact a function of random variation times natural selection, but whether or to what extent Fisher has demonstrated that it is so. The crucial text we have to consider is chapter two, on the Fundamental Theorem, although much that is said in the chapters on the evolution of dominance and on mutation and selection is also relevant.

What is to be demonstrated in chapter two, Fisher tells us at the start, is that *the rôle of improvement of any species of organisms in relation to its environment is determined by its present condition.* 'Improvement in relation to environment' seems to mean 'increased adaptation'; but the term 'adaptation' is formally introduced only later, where it is defined in terms which appear to presuppose the concept of 'improvement':

> An organism is regarded as adapted to a particular situation, or to the totality of situations which constitute its environment, only in so far as we can imagine an assemblage of slightly different situations, or environments, to which the animal would on the whole be less well adapted; and equally only in so far as we can imagine an assemblage of slightly different organic forms, which would be less well adapted to that environment.[2]

In other words, an organism is regarded as adapted, if one can imagine its condition as an improvement over another possible condition that would be slightly less favourable.

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[1] Sir Ronald Fisher, *The Genetical Theory of Natural Selection*, New York, 1959, p. 38. I am following the Dover edition, which is in part revised, but not, with one exception, to be noted later, in ways that are philosophically relevant.

[2] Op. cit., p. 41

Thus the concept of improvement is central to the argument, and we must find out what it means. And there is also the question, in what sense the present condition of an organism 'determines' its 'improvement in relation to its environment'. Does 'determine' here mean 'cause'; if so, how, or if not, what does it mean?

3

Fisher's procedure is to establish a table of reproduction, analogous to a life table, and to specify a parameter of population increase *m*, expressing the 'relative rate of increase or decrease of a population when in the steady state appropriate to any such system',[1] that is, when the distribution of age groups in the population is constant. Fisher calls this the 'Malthusian parameter of population increase', and it is said to 'measure *fitness* by the objective fact of representation in future generations'.[2] Now suppose we are measuring a particular character, such as tallness, in a population. And suppose we assume, as in the light of orthodox genetics we do, that all persistent changes in organisms are produced by changes in their genetic make-up.[3] Then we may calculate the average effect on the character in question of a given gene substitution, and also the effect of this one gene substitution on *m* (that is, on the chance of leaving posterity). We then calculate a summation of these gene changes *and* their effects on *m* for all the genes in the population, and we get what Fisher calls the 'genetic variance'—in this case for example, the genetic variance of tallness. Now suppose further that what we are measuring is not tallness but fitness itself. We then find, by further calculation, that : '*The rate of increase in fitness of any organism at any time is equal to its genetic variance of fitness at that time*'.[4] This statement Fisher describes as 'the fundamental theorem of Natural Selection'.

'*m*', or the 'rate of progress of a species in fitness to survive', is, as Fisher says, 'a well-defined statistical attribute of a population'.[5] It is the increase (or decrease) in the chance this population has of leaving posterity at some future date. 'Progress' here means simply this statistical increase or decrease, and 'fitness' in this context would seem to be the same for all organisms: it is a statistical measurement, which has, so far, nothing to do with the particular structures or functions of particular organisms as suited to particular environments. In fact, it has, so far, nothing to do with environment at all, nor with 'improvement' in any sense other than the very restricted quantitative one: that a gene or an individual or a population may be said to be 'improved' when its chance of having descendants at some future date has risen. This is 'improvement' in the sense in which a patient has improved when he is more likely to survive.

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[1] Op. cit., p. 26

[2] Op. cit., p. 37

[3] I am speaking here (as Fisher is doing) of genic inheritance. The question of cytoplasmic inheritance introduces another dimension altogether into the evolutionary problem.

[4] Loc. cit. (my italics)

[5] Op. cit., p. 40

4

If this were the whole story, there would be no problem. It would be possible, as indeed happened far and wide in the wake of the publication of Fisher's book, to make valuable calculations, resembling actuarial tables, by which to record and, up to a point, to predict the gradual increase or decrease of certain genetic factors in Mendelian interbreeding populations. Some of the great array of experimental work in which Fisher's techniques were in fact applied is referred to in this edition.

It seems odd, however, to speak in this connection of 'improvement in relation to environment' or of 'Natural Selection'. 'Improvement in relation to environment' suggests a correlation which has so far not been considered; and similarly, 'Natural Selection' seems to describe not a purely numerical relationship, but something qualitative as well; that is, the elimination of characters less well adapted to a particular environment in favour of those slightly better adapted to that environment.

In what sense, then, is the Fundamental Theorem a theorem of '*Natural Selection*' (concerned, *a fortiori*, with 'improvement in relation to environment')? In introducing the notion of 'reproductive value', that is, the probability that a given individual or a given age will produce descendants in respect to some future date, Fisher remarks that this value is of interest, since '*the action of Natural Selection must be proportional to it*'.[1] This is indeed true. If, as Fisher points out, the reproductive value of the inhabitants of Great Britain decreased between 1911 and 1921 as a result of the First World War, then whatever selective effects *might* take place, *must* take place only upon that declining group; and therefore the action of Natural Selection would be proportional to the decline. And conversely, where there *is* Natural Selection taking place, its operation can be investigated and measured by the use of Fisher's techniques. The fact of industrial melanism, for example, has been known for many years; but Kettlewell has recently studied it with great care and precision in terms of modern population genetics.[1] Thus if normal peppered moths (*Biston petularia*) are taken by predators in industrial areas in greater numbers than the darker *carbonaria* mutants, then the increase of *carbonaria* and the decrease of light coloured individuals can be recorded as a decrease in reproductive value of the normal form (which is more likely to be eaten before reproducing—and its offspring also and so on) or an increase in the reproductive value of the *carbonaria*, that is, an increase in the probability that there will be *carbonaria* in future. In other words, once we know that selection has *in fact* taken place in a particular situation, we can record and elaborate this knowledge in the statistical terms of Fisher's theory, and within the limits of selection experiments we can apply Fisher's technique to predict their results.

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[1] Op. cit., p. 27

This statement about reproductive value, moreover, holds equally for the fundamental theorem itself, which generalises the notion of increase in reproductive value for an active interbreeding population, and equates it with the increase in probability of survival of the particular genes forming the genetic constitution of the population in question. Thus *either* (as in the decline of the British population) the fundamental theorem expresses the limits within which Natural Selection can operate; *or* (as in the case of industrial melanism) the occurrence of Natural Selection (as known from other sources) entails the truth of the fundamental theorem.

5

But it is not at all clear that the fundamental theorem entails Natural Selection. In fact, in the example of reproductive value cited by Fisher, the effect of war, the change in reproductive value is notoriously one in which what would appear to common sense to be the better adapted are eliminated in favour of the less well-adapted: the very opposite of a natural selection effect. For natural selection, according to Fisher's own insistence here and elsewhere, expresses the tendency to

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[1] H. B. D. Kettlewell, *Nature*, 1955, **175**, 943; *Heredity*, 1955, **9**, 323, and 1956, **10**, 287; *Proc. Roy. Soc.* B, 1956, **145**, 297

increased, not to *decreased* adaptation.[1] More generally, we should recall also that, as Darwin himself admitted, natural selection is applicable only to such characters or functions of living things as are both heritable and adaptive (whether for good or ill)—that is, relevant to viability or the reverse in a given environment. But we might record the increase or decrease of all sorts of characters in populations without regard to their 'adaptive' value one way or another. Suppose, for example, that red hair is increasing, and hence the probability of larger numbers of future redheads is also increasing; this does not in itself tell us anything about the usefulness of red hair, unless we know from some other source that all characters that are increasing in the population must be of use to it. Darwin was well aware of this restriction on his theory, but was confident that on the whole most characters of organic beings are adaptive. And what Darwin demonstrated in the first four chapters of the *Origin* was that, given the largely adaptive nature of organisms, and variation, and inheritance, and Malthusian population increase, natural selection necessarily follows.[2] But Fisher's statistical proof does not appear to be rich enough in its premises to permit this conclusion. In terms of the 'strictly defined' concepts it uses, the fundamental theorem is not a theorem of natural selection, but a statistical device for recording and predicting population changes. Nor is the situation altered by calling such changes 'genetical selection'. We must still distinguish between 'genetical selection', which is purely statistical, and Darwinian selection, which is environment-based and causal. They remain two distinct concepts with a common name.

6

It is important to keep this distinction in mind when interpreting the use made by Fisher himself and by others—notably, for example, Huxley, Haldane, Simpson—in applying his formula. The fundamental theorem has been stated, Fisher says, for 'idealised populations' in which 'fortuitous fluctuations in genetic composition have been excluded'.[3] He then calculates the error due to such 'fortuitous fluctuations'. The standard error turns out to be $\frac{1}{T}\sqrt{\frac{W}{2n}}$, where T is the time of a generation, n is the number of the population, and W the rate of increase in fitness of a population measured in terms of the summation of the effect of the increase in particular genes on m. In terms of this formula, it turns out that the rate of increase in fitness becomes irregular only when it is so slight as to be of the value of $1/n$. Only then, in other words, do the random fluctuations produced in a single generation by mutation (given random mating) affect the exactness of the statistical measurement, or, as Fisher puts it, 'the regularity of the rate of progress'. And more than this: if a long enough time is allowed, even this irregularity will recede to vanishing point. For even if the value of m for different genotypes were so delicately balanced in a given generation as to show only a $1/n$ distinction, and hence a fluctuation in m and the summation of the effects of genes on m, still over a span of 10,000 generations, the deviations from regularity would be just a hundredfold less.

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[1] Or if, alternatively, we say that in war it is the ('normally') mal-adapted who are *better* adapted, and vice versa, then we are using 'better adapted' to mean 'surviving' and are saying only: 'those who survive, survive'.

[2] The demonstrative character of Darwin's argument was pointed out by C. F. A. Pantin (in *History of Science*, London, 1953; cf. the discussion by A. G. N. Flew, in *New Biology*, 1959, no. 28, pp. 25 ff.

[3] Fisher, op. cit., p. 38

Now this argument is taken to show that 'very low rates of selective intensity' are effective in nature. But if we have interpreted the fundamental theorem correctly, what it shows in fact is that very slight trends in the frequency of characters in populations may be recorded if they persist over a long period of time. Whether, in nature, such trends are the result of a process that can reasonably be called 'natural selection' is another question altogether.[1]

How does Fisher deal with this question? What he argued in his 1932 paper in *Science Progress*[2] was that such trends are not, in large populations, the result of mutation alone (or even chiefly), since mutations are too infrequent and advantageous mutations still more infrequent, and therefore it must be natural selection that directs the trends which his statistics describe. This follows, however, only if mutation and natural selection are the only two possible causes of evolutionary change: and that, Fisher is arguing, is what the theory is supposed to prove, not to presuppose.

Nor is the argument of our present text (i.e. the *Genetical Theory*) more satisfactory, for it moves in an even narrower circle.[3] Since only mutation rates around the $1/n$ value are ' selectively neutral ', Fisher says, random variation, i.e. mutation, will very seldom indeed be an effective factor in evolution. But what does ' selectively neutral ' mean? It means precisely: with a mutation rate, that is, a rate of gene substitution, around the $1/n$ value or less. Similarly, ' 1 per cent selective advantage ' *means* 1 per cent increase in numbers of a particular gene substitution. Any mutation rate more substantial than $1/n$ therefore automatically *becomes* ' selective advantage ' and so the product of selection, not mutation; and any lesser mutation rate remains ' selectively neutral '; that is, counts as mutation, not selection. All this will be a more than verbal argument only if we know from other evidence that selection, in the environment-related, Darwinian sense, is always the cause of such statistical advance. In short, genetical selection is entailed by and measures but does not entail Darwinian selection.

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[1] If one has set up a selection experiment, of course they are so.

[2] R. Fisher, 'The Bearing of Genetics on Theories of Evolution', *Science Progress*, 1932, 27, 273-287

[3] Ibid., pp. 131-132

In terms of the concepts used in the fundamental theorem, as strictly defined, therefore, the traditional theory of natural selection, that is, improvement in relation to environment necessitated by environmental pressures, has not been touched on at all. Increase or decrease in ' fitness ' or if you like ' improvement ' in Fisher's strict sense, is either the measure of the effect of Darwinian selection if there happens to have been any, or the measure of whatever else may have been happening to bring about increase in certain genetic factors rather than others. Similarly, what Fisher calls ' the *progress* of a population in fitness to survive ' again means strictly no more than the increased probability of leaving posterity, whatever may be the reasons. There is no justification, in terms of the concepts ' as strictly defined ', for considering this as ' progress ' in any other sense.

7

Let us return now for a moment to Fisher's opening statement (at the beginning of Chapter 2) and put it alongside the fundamental theorem.

> The improvement of any organism in relation to its environment is determined by its present condition. . . . The increase in fitness of any organism at any time is equal to its genetic variance in fitness at that time.

In the context of Fisher's argument, ' improvement ', etc., clearly refers to the increased probability of leaving descendants, *for whatever reason*. And ' present condition ' means present genetic variance in fitness, that is, in the probability of leaving descendants. In other words, it means the summation of such probabilities itemised in the gene pool of the relevant population.

What does it mean, finally, to say that the second of these '*determines*' the first? The fundamental theorem asserts that the rate of increase in fitness of any organism at any time is exactly equal to the genetic variance in fitness at that time. But Fisher has just said (in his proof of the fundamental theorem) that the rate of increase in fitness here means rate of increase in fitness *due to all changes in gene ratio.*[1] Now the genetic variance in any measurement in population means, according to Fisher's earlier definition[2] the summation, for all genes, of the effects on *m* of the excess of any one gene over its alleles. But this *is* precisely the summation of the effect on *m* of all changes in gene ratio. To say that the rate of increase in fitness is due to changes in gene ratio is to assert a fundamental belief of modern genetic theory. To identify the increase so caused with the 'genetic variance in fitness' is to assert an identity. How can such a statement be said to 'determine' anything? We have present rate of increase in fitness = improvement; genetic variance in fitness = present condition. Either the second determines the first in the sense that we *are* simply stating an identity; and this tells us nothing about organic phenomena except as formalising what we already know—that, as Professor 'Espinasse has put it,[3] wherever there are characters there are *some* genes that cause them. Or else the fundamental theorem is meant to direct our attention to the tables of reproduction which can be used to chart the trends in populations both at the level of individuals and at the genetic level, and to correlate trends in other measurements with trends in *m*. We could say, for example, that the overall increase in tallness of a population is equal to its genetic variance in tallness, because we believe that whatever trend a population is showing in respect to a given measurement has some genetic basis. To say that its overall trend in fitness is equal to its genetic variance in fitness is not to express any further 'determination' over and above this. Again, the only determination expressed here is the determination we know of from genetics, and the statistical measurements of *m* add nothing to this. There *may* of course be other causal connections in nature (in predator-prey situations and so on), and our statistical measurements may guide us in our analysis of, and perhaps even our search for, these. But in themselves statistics cannot specify such connections. In other words, the fundamental theorem is a guide to statistical technique which is overlaid on the causal relation of heredity and can be used as underpinning for the causal study of Darwinian selection; but in itself it asserts neither.

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[1] Op. cit., p. 37

[2] Op. cit., pp. 30–32

[3] In an address to the British Society for the Philosophy of Science in February 1959

8

But how can the imposing edifice of modern neo-Darwinian theory rest on so narrow a base? In fact, it does not. It rests on the broader foundation of Darwinian thinking which is drawn into the circle of Fisher's statistical theory in virtue of the ambiguity of its central concepts.

We have only to look at Fisher's comments on the fundamental theorem to see how this works. He likens this principle to the second law of thermodynamics, as a statistical law that reigns supreme over a vast area of nature. But fitness, he says, though measured by a uniform method, ' is qualitatively different for every organism '.[1] Now this is not fitness in the sense of the mathematical chance of leaving descendants, which is a quantity, and cannot be ' qualitatively different ' for each organism. On the contrary, Fisher is here referring to fitness in the sense which he goes on to amplify later in the same chapter, in the section on ' the benefit of the species ': that is, fitness in the qualitative sense of an ' advantage ' (of a particular kind) to the individual organism.[2] ' Fitness measured by m ' will again become extremely important in the theory of the evolution of dominance (in chapter 3) where the biologist's concern is with the probability, for certain genetic factors, of leaving a remote posterity, even though these factors may lie deeply hidden in the present population, phenotypically considered. But here (in the latter part of Chapter 2) the statistical concept of fitness has been translated into old-fashioned Darwinian fitness: in terms of our previous example, the benefit to a Manchester moth of being black and the harm to his cousin of being speckled. Of course if mostly peppered moths are eaten and black ones spared, the population will come to include more black and fewer speckled individuals. But the statistical result to the population in the future and the immediate benefit to this sooty moth on this grimy tree trunk today are not the same thing. Yet through the identity of the word ' fitness ', the insistence that selection has to do with present advantage, not ' trends ', is here attached to Fisher's original, stricter concept of ' fitness ', which thus becomes Darwinian as well as genetical and immediate as well as long-run. So we have genetical selection for later and Darwinian selection for now fused under a single word ' fitness '.

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[1] Op. cit., p. 39

[2] This section is added in the second edition, but what Fisher says here is implicit in the usage of the first edition also.

And ' progress ' is equally elastic. We have ' progress ' in fitness-measured-by-m: for example, progress in the height of the population if tall individuals are on the increase (never mind why); and at the same time we have progress in an advantageous character in the sense of increased adaptation, if, say, tallness (as in the proverbial giraffe) actually, for some reason, causes its possessor to get more to eat than his shorter brothers and therefore causes, rather than simply measuring, survival.

9

There is more to it than this, however. The advantage to the individual organism on which Fisher insists is not just the traditional one of an advantage in facing predators or the like, that is, as against our contemporaries, a lesser chance of death. It is also (and must be, in an evolutionary context) the chance of reproduction. ' It will be observed ', Fisher writes,

> that the principle of Natural Selection, in the form in which it has been stated in this chapter (i.e. the fundamental theorem) refers only to the variation among individuals (or co-operative communities), and to the progressive modification of structure or function only in so far as variations are *of advantage to the individual in respect to his chance of death or reproduction*. It thus affords a rational explanation of structures, reactions and instincts which can be recognised as profitable to their individual possessors. *It affords no corresponding explanation for any properties of animals or plants which, without being individually advantageous, are supposed to be of service to the species to which they belong.*[1]

Now if we omit the qualification ' or reproduction ', this is a fair statement of the situation described by Darwin. Where the benefit of individuals is concerned, it is the chance of death that selection controls. True, we may also infer that, since the chance of reproduction is obviously tied to the chance of death, the future numbers of organisms possessing immediately advantageous characters, given certain environmental conditions, will increase in numbers also, if and when those conditions persist. Moths that are eaten cannot reproduce thereafter. But the *immediate advantage* to the moth in this case is not reproduction, but the omission of being eaten. Reproduction would be an advantage to the moth's descendants, and indirectly to the moth in so far as it is advantageous to it to satisfy its instincts; but in the situation of pure Darwinian selection—as in this case of the black moth on the black tree trunk—it is the character that keeps one alive that counts. Reproduction as the continuation of the species is a matter that is indirect and inferred; it is staying alive that is the immediate benefit.

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[1] Op. cit., p. 49 (my italics)

Yet the ' or reproduction ' alternative is essential to Fisher's *genetical* selection—and again, to the future-directed character, in particular, of the evolution of dominance: where in present-day recessives—which may some day become dominant—we are dealing, as he says himself, with the ' chance of leaving a remote posterity ' by storing up characters that will some day be useful in some distant future environment. Such a chance is surely meaningless in terms of ' advantage to the individual ' unless in some Biblical sense, that a man feels he is cut off from immortality if he is cut off from having living descendants. In short, we have tied together in the concept of fitness three kinds of ' selection ': genetical selection, which is future-directed but only in a Pickwickian sense selection at all; Darwinian selection for now, directed to immediate advantage; and Darwinian selection for later, in reference to future advantage. The first specifies progress in the purely statistical sense of increased chance of survival of some genes rather than others; the second in the sense of short-run increase in adaptation; the third, long-run increase in adaptation.

Finally, added to these, we must notice another of Fisher's comparisons between his theorem and the Second Law. While physical systems run downhill, he says, evolution tends on the whole to produce ' progressively higher organisation of the organic world '.[1] This is progress in a new sense, and one which escapes all Darwinian considerations, though again its existence, once admitted on other grounds, might be recorded with the help of statistical methods.

Thus the larger conceptual structure of the theory is both richer and less exact than the proof of the fundamental theorem would lead us to expect. It consists of a network of concepts, in which statistical and deterministic, genetical and Darwinian meanings, short-run and long-run assessments, reinforce one another in a self-confirming circle.

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[1] Op. cit., p. 40

For if fitness means, after all, advantage to the individual, then the long-run trends of reproduction tables *must* represent real benefits in each generation, and if at the same time fitness *means* the statistical progression measured by *m*, then such progression must entail the whole causal context that Darwinian 'fitness' implies. It is only in this way that the 'determination' mentioned in Fisher's initial thesis can be said to make sense. And similarly, the references to internal or genetical and external or Darwinian 'selection' and the references to future (genetical or Darwinian) and present (Darwinian) 'fitnesses' confirm one another, in circular re-inforcement, indefinitely.

10

This ambiguity will become clearer if we pin-point the narrower and wider senses in which the concept of 'improvement' occurs in the argument and consider their evolutionary import.

First, there is the strict statistical meaning of the Malthusian parameter: the increase in the probability that organisms possessing certain characters will leave offspring. Such an increase, though calculated for an infinitesimal period, has meaning only over time. What it asserts about organisms in time may be taken in two ways: either as a tautological statement: what has survived has survived, what is on its way to surviving is on its way to surviving; or as the retrospective appraisal of an achievement: what has survived is not what has failed to survive, but what has succeeded. But this says nothing of *why* that might have happened, either in terms of adaptive relations to environment or anything else. Tables of reproductive value record the multiplication of some characters and the disappearance of others; that is all one can say in these terms. And in terms of these tables, one may assert the probable increase of some genes rather than others.

In this sense of 'improvement' Fisher is right in saying that organisms must show 'improvement' up to the moment of extinction. In terms of the increase of some genes as against their alleles, they doubtless do. For since the sum of any gene and its alleles in any population always = 1, we can always take the increasing rather than the decreasing gene (or genes, where there are multiple alleles) and so get a picture of 'improvement'. This is really why, in the fundamental theorem, the calculated value is always positive. Not only is the time interval, *dt*, necessarily positive, but the changing gene ratios also can always be taken in this sense.

II

Alongside this statistical formulation, however, consider what Fisher says about the universality of ' improvement ' in his comment on the fundamental theorem. His first comparison with thermodynamics is to this effect:

> The systems considered in thermodynamics are permanent; species on the contrary are liable to extinction, although biological improvement must be expected to occur up to the end of their existence.[1]

The phrase ' biological improvement ' brings us back to the ' improvement in relation to environment ' from which we started; it is what Fisher describes elsewhere as ' progress determined by Natural Selection ', the full Darwinian improvement so emphatically underlined in the second edition. According to this conception, organisms are in fact constantly becoming better adapted to their niches in nature, as natural selection weeds out the imperfectly adapted; and therefore it follows, further, as Fisher in fact argues in some detail, that the explication of all cases of extinction must be referred to deterioration of the environment. Thus when the dinosaurs died out, for example, something must have gone wrong in their environment to bring this about. That this is actually so in nature, however, cannot be inferred from the fundamental theorem by taking ' improvement ' in its strict and statistical sense.

We are dealing here, in other words, with a second concept of improvement. We are asserting that there is improvement in the sense of the appearance of adaptive relations with immediate effectiveness here and now, characters or functions which are genuinely advantageous in relation to their bearer's actual environment, and which may therefore, as compared with their absence, be called ' improvements '. These are the improvements effected by Darwinian selection, and the concepts presupposed in the assertion of such improvement are not by any means the same as those supporting the statistical concept. While statistical ' improvement ' entails the conception of the organism as an aggregate of gene effects, Darwinian improvement entails in addition the conception of the organism as a machine with parts adapted to the performance of their special functions. The improvement that is effected here is that of increasing adaptation in the sense of specialisation, of fitting in better and better to a special niche in nature. The evolutionary import of such improvement is *katagenetic*, to use a term introduced by the German evolutionist Rensch. That is, it is evolution downhill, in the sense that species break up into varieties and subspecies as specialised demands are made on them. Darwin's finches are a classic example.

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[1] Op. cit., p. 40. For an excellent discussion of the logical place of biological improvement in Darwinian and neo-Darwinian theory, see the article by Flew mentioned in note 2, p. 30 above.

As against genetical 'improvement', which is meaningful only as an assessment of a trend over a lapse of time, this kind of improvement is short-run. On the other hand, it is not intelligible, as the statistical concept purports to be, in terms of gene ratios alone. Its assessment depends on the recognition of phenotypes, as wholes, not as aggregates of genes, and on the recognition of the relation of such wholes to their environment, to predators, to climate, and so on and so forth. It is neither future moths, nor gene pools, to whom it is advantageous not to be taken by a bird, but this black moth on this tree trunk today. And the evolutionary trend which establishes and maintains a phenomenon like melanism expresses the accumulation of millions of such individual escapes and individual disasters, not to genes or gene pools, but to moths, whether today or yesterday or (from some cause other than the industrial revolution) a million years ago.

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So much for our first pair of 'improvements'. However, as critics of natural selection theory have long been saying, there also seem to be adaptive relations which develop only slowly, and which do not appear to benefit their possessors at the beginning of their development. Such long-run adaptation appears to entail a third meaning of 'improvement'. It refers to characters or functions which will be 'better' for future phenotypes in future environments. Such improvement we may call *quasi-anagenetic*; it still concerns particular adaptive relationships and specialisations, but specialisations which accompany the emergence of new forms (rather than new sub-styles of old forms) and which develop slowly over a very long period of time. Now this seems to be the kind of situation which Fisher explicitly excludes, exiling the 'benefit of the species' approach as teleological and irrational. In terms of what he says there, one would conclude that all ultimately useful characters must have been in some direct way useful even in their minute beginnings. And it is true that this has been shown to be possible or even likely in a number of cases, for instance, primitive photoreceptors, electric organs, feathers, and so on. One can, if one likes, extrapolate such instances to all cases of clearly adaptive characters. Yet the forward reference, as distinct from immediate utility, seems essential to Fisher's own type of statistical selection, and in particular to changes in the relations of recessive and dominant alleles. Here we have recessives, either useless or even harmful, hidden away in a population over a long period of time—up to the moment of a new environmental situation which makes them advantageous and so calls forth modifiers that turn them into dominants. Only the reference to 'remote posterity' makes sense of this story. True, there is the case of sickle-cell anaemia: where we have a gene which is lethal in the homozygote, but in the heterozygote actually gives protection against malaria, and is kept going in the population by this beneficial effect. Supposing a situation in which sickling were no longer harmful, we could imagine that a character now maintained, but in a recessive state, because of the advantageous nature of the heterozygote, might become dominant and pervade the population much more completely. And we might then extrapolate this kind of process to all cases of the retention of recessives apparently for the sake of their future usefulness, but really for the sake of some other present benefit. Yet even then, the modifiers which will eventually make the now-recessives dominant must be lurking in the population ready to leap into action when the environment demands, and thus the future-directedness of the whole procedure is simply transferred to them. It seems strange to say, as Darlington does, that the genes are endowed with an automatic property of foresight,[1] yet the reference to 'remote posterity' which dominat Fisher's argument on dominance does indeed suggest some such ide

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How can Fisher and other evolutionists who follow him remai happily unaware of this third and uncomfortably long-term 'improvement'? What happens, I think, is something like this. We have so far, three concepts of 'improvement'. *Improvement 1, statistical selection,* is the measure of *improvement 3, long-run adaptation.* But long-run adaptation is *adaptation,* and where there is adaptation there must, in the light of the Darwinian theory of natural selection, be *improvement 2, Darwinian selection*—for Darwin has proved that that is how adaptation is produced. So improvement 3, which is in fact unintelligible in terms of the dictum of improvement 2 (since in its terms all adaptation bears on immediate, not remote benefit) is nevertheless subsumed under it through the measurement of both of them by the techniques of improvement 1. Moreover, improvement 1 is expressed by a differential, which can be interpreted as a summation of gene changes *now*—short-run trends—or over as long an interval as you like—long-term trends; but at the same time, since it is one statement (the fundamental theorem) it must express one relationship, and since in Darwinian terms improvement 3 would be nonsense, this one relationship must be the situation covered by improvement 2.

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[1] C. D. Darlington, *Evolution of Genetic Systems,* 2nd edn., Edinburgh, 1958, p. 239

The way in which 1, 2 and 3 are assimilated to one another is most evident if we place the argument about the fraction $1/n$ and the efficacy of very slight 'selective intensities' alongside the argument about immediate advantage. As Fisher himself argues, irregular variation in the rate of increase in fitness as measured by m is more apparent in a single generation than over a longer period, and therefore very small 'selective values' are sufficiently strong to establish themselves *over a long enough time*. This statistical observation is often used against those who object to natural selection theory because of the difficulty of accounting for the first beginnings of what will ultimately be useful traits. Selection can do so much *because it has so long*.[1] Yet if one speaks of trends in evolution, of orthogenesis or the like, one is told that this is nonsense because evolutionary modification always consists in the selection of the immediately useful at each step, in each generation.[2] So improvement 3, long-run adaptation, with its statistical measure in improvement 1, is used to answer one objection, while improvement 2, which can also be expressed by the same statistics, applied to short-run situations, is invoked to answer another.

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Finally, we must mention a fourth meaning of improvement, the kind of improvement which is truly an advance to higher forms of li Such improvement is explicitly mentioned only in Fisher's r k about his fundamental theorem and the tendency to 'higher o ation' (as contrasted with the Second Law and entropy). This is ly kind of improvement which represents true *anagenesis* in the

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s example, the contributions of Huxley and Fisher to the Huxley, Hardy,

Fc nc referred to in note 2, p. 25 above, or Huxley's *Evolution in Action*, 1953.

sense of emergence, or the appearance of genuine novelty at a higher level of richness or complexity. But this is also a kind of improvement which neither statistical genetics nor selectionist biology can handle, since it is neither quantitative nor adaptive. It is best for selectionists to ignore it, as Darwin warned himself to do when he wrote in his copy of the *Vestiges* 'Never speak of higher or lower in evolution'. Yet the great outlines of the fossil record are there, and demand to be spoken of, especially since the fact that we can speak of them is one of the surprising results of the process they record. But evolution as macro-evolution, as the emergence of life and of higher forms of life, outruns both the concept of gene-substitution, and of improvement in relation to environment. It makes sense only as an achievement—an achievement for which statistical methods can measure the necessary, but not the sufficient conditions.

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One brief concluding remark. I have side-stepped here altogether the question of prediction and retrospect, of the historical nature of evolutionary explanation: a question which is very close to the philosophical difficulties raised by Fisher's theory. Evolutionary theory is essentially an assessment of the past. Fisher treats it in terms of present and future. Just how closely the philosophical confusions of this kind of argument are related to the attempt to think unhistorically about an historical subject matter, I should not at the moment venture to say.

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A CAUSAL CALCULUS (II) *

I. J. Good

6 *Two-state Markov Processes*

The radioactive process described in Axiom 23 can be slightly generalised by permitting return from the black to the white state, with a parameter β corresponding to the α of the white-to-black transition. We have a two-state Markov process with continuous time. The parameters α and β are of course both non-negative. In the special case of the radioactive particle we have $\beta = 0$.

It can be shown that

$$Q(E : F) = \log\left((\alpha + \beta e^{-(\alpha+\beta)T})/(\alpha - \alpha e^{-(\alpha+\beta)T})\right).$$

If the particle ever entered the black state during the time interval, T, the chain would be cut and the degree of causality would be zero. Assuming that this does not happen, we can calculate $\chi(E : F)$ by applying a Riemann dissection to the interval, so as to obtain a causal chain consisting of a finite number of events, and then proceed to the limit as the fineness of the dissection tends to zero. By applying T17 and A9 we find that

$$\chi(E : F) = -\log(1 - e^{-\alpha T}),$$

which is mathematically independent of β.

For large T, both Q and χ are exponentially small, but Q is smaller than χ, and is much smaller if β is large. This is reasonable since, if β is large, the initial state makes little difference to the probability of being in the white state at the end of the interval.

Note that χ is the degree to which being in the white state rather than in the black state at the end of the interval was caused by being in the white state rather than in the black state at the start of the interval. A similar explicit description can of course be given for Q.

7 *Partially Spurious Correlation*

A well known pitfall in statistics is to imagine that a statistically [illegible]ificant correlation or association is necessarily indicative of a causal [illegible]tionship. The seeing of lightning is not a cause of the hearing of thunder, though the two are strongly associated. Such associations and correlations are often described as ' spurious ', a better description than ' illusory '. They may also be *partially spurious*, and the explicata for Q and χ should help with the analysis of such things. Smoke and dust might be a strong cause of lung cancer, but smoking only a weak cause. Even so, the correlation between smoking and lung cancer may be high if there is more smoking per head in smoky districts. I mention this only as an example, and have not made a special study of this problem.

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* The first part of this article appeared in the previous Number.

Note that

$$Q(E : F . G/\bar{F} . \bar{G}) = Q(E : G|\bar{F}) + Q(E : F|G),$$

so that the tendency to cause can be split into components, somewhat in the manner of an analysis of variance. For example, the tendency for lung cancer to be caused by smoking and living in a smoky district as against not smoking and living in a clean district is equal to the tendency through living in a smoky district, given no smoking, plus the tendency through smoking, given that the district is smoky. It is also equal to the causal tendency through living in a smoky district, given that one smokes, plus the tendency through smoking, given that the district is clean. This approach to the analysis of spurious correlation is entirely different from, and more quantitative, than the approach used by Simon.[1]

Let

$$K(E : F) = - I(\bar{E} : F),$$

the *intrinsic* causal tendency of E by F. It is related to Q in essentially the same way that I is related to W, since

$$Q(E : F) = K(E : F) - K(E : \bar{F}),$$
$$Q(E : F/F') = K(E : F) - K(E : F').$$

K does not depend on the negation of F, so its use enables us to avoid the distribution, D, of Section 4. We have

$$K(E : F . G) = K(E : F) + K(E : G \mid F),$$

so that K can be split up into contributions from various sour simpler manner than Q. In my opinion both K and Q will pr have useful applications in statistics and physics.

The remainder of this paper is primarily concerned with the e sion of the explication of causal strength to general nets, in order degree of causality should be generally explicated. The next sec however contains a formal definition of a causal chain, which str was required in what has already been discussed. I postponed it in order not to interrupt the thread of the argument.

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[1] Herbert A. Simon, *Models of Man*, New York and London 1957

8 *Causal Chains* [1]

Let $F = F_0, F_1, \ldots, F_{n-1}, F_n = E$, be $n + 1$ events such that (for $i = 0, 1, \ldots, n - 1$):

(*i*) F_i and F_{i+1} are contiguous in space and time, or approximately so.

(*ii*) No two of the events overlap much in space *and* time.

(*iii*) All the events occurred (or will have occurred, i.e. they 'obtain' but I prefer to write simply 'occurred').

(*iv*) F_{i+1} started later than F_i did.

(*v*) F_i had a positive tendency to cause F_{i+1}.

(*vi*) If F_i is given, then the probability of F_{i+1} is unchanged if one or more of the earlier events did not occur, i.e. we have a Markov chain.

(*vii*) If the chain is embedded in a completely detailed chain containing intermediate events, then condition (*v*) will remain true for the more detailed chain.

Then we say that $F_0, F_1, \ldots, F_n$ or $F_0 \rightarrow F_1 \rightarrow \ldots \rightarrow F_n$ is a causal chain connecting F to E. Perhaps it should be called a 'putative causal chain' if condition (*vii*) has not been established. In practice all causal chains are putative, but there are degrees of putativity.

The failure of condition (*v*) may be said to 'cut the chain'.

A causal net will be formally defined in Section 11. A chain is a special case of a net.

9 *Independent Causal Tendencies*

Let $G_1, G_2, \ldots, G_m$ be independent given H, and also independent given $H . \bar{E}$. Then it is easily proved, with the help of T15, that the tendencies to cause E are additive in the sense of the theorem below. It therefore seems reasonable to say in these circumstances that the G's have *independent tendencies to cause* E *given* H. The events G_1, G_2, and G_3 of A20 exemplify this definition, with $H = F$, and also with $H = \bar{F}$; that they are independent given $\bar{E}$ is trivial since their probabilities are then all zero.

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[1] Cf. Hans Reichenbach, *The Direction of Time*, Berkeley and Los Angeles 1956, Index reference under 'Causal chain', and *The Philosophy of Space and Time*, New York and London 1956, Index reference under 'Casual chain' [*sic*]

T18. *If* $G_1, \ldots, G_m$ *have independent tendencies to cause* E *given* H, *then*

$$Q(E : G_1 . G_2 . \ldots . G_m \mid H) = \sum_i Q(E : G_i \mid H).$$

The nets of A20 and T10 also exemplify the following definition :

A *bundle of parallel independent causal chains* from F to E is a class of chains from F to E such that, apart from F and E, each event on each chain is, given F and given $\bar{F}$, probabilistically independent of any collection of events on other chains, and also such that the penultimate events have independent tendencies to cause E, given their pasts.

10 *Series-parallel Networks*

As an extension of T11 it is natural to define the strength of a bundle of independent causal chains as the sum of the strengths of the individual chains.

For a ' chain of bundles ', in a self-explanatory sense, we can first calculate the resistance by summing the resistances of the individual bundles, and then obtain the strength from T16. We can extend the process to bundles of chains of bundles and so on. In other words we can construct natural rules for evaluating the causal strength of any ' series-parallel ' net. Topologically these are the same as the two-terminal series-parallel networks whose enumeration was considered by MacMahon.[1] Not all networks are of this type.

11 *Causal nets ' Having Independence '*

Let $\mathfrak{n}$ be a class of events all of which occurred. For each event, G, in $\mathfrak{n}$, there is a subclass of earlier events, $G_1, G_2, \ldots, G_k$, which so to speak, ' lead in ' to G. By ' lead in ' is meant that the probability of G, given which of $G_1, G_2, \ldots, G_k$ occurred and which did not, is independent of any further assumptions of which other events in $\mathfrak{n}$, earlier than G, occurred. (Note that not all the events in $\mathfrak{n}$ are regarded as ' given ' even though they all actually occurred. This should cause neither surprise nor confusion to those who are familiar with the idea of a conditional probability.) We may think of k oriented links joining $G_1, G_2, \ldots, G_k$ to G. If the whole class, $\mathfrak{n}$, is connected together by means of such links we describe $\mathfrak{n}$ as a *causal net*. If E is the latest of the events in the net, and can be reached from each other event by passing through a succession of links in the right direction, then the causal net will be said to *lead to* E. If F is the earliest of the events in n, and each other event can be reached from F by passing through a succession of links in the right direction, then the causal net will be said to *lead from* F. If both conditions are satisfied, the net will be said to *lead from* F *to* E. For example, a net leading to E could have the form of a ' tree ', but a net leading from F to E could be a tree only if it were a chain.

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[1] P. A. MacMahon, ' The combination of resistances ', *The Electrician*, 1892, 28, 601–602. Or see John Riordan, *An Introduction to Combinatorial Analysis*, New York and London 1958, pp. 139–143.

In this definition we may call $G_1, G_2, \ldots, G_k$ the *immediate predecessors* of G. A causal net will be said to *have independence* if, for each G in the net, the immediate predecessors have independent tendencies to cause G given the past.

For each link $G_i \rightarrow G$, having a ' p ' and a ' q ',

$$p = p_i = P(G \mid G_i),\ q = q_i = P(G \mid \bar{G}_i),$$

let the *quasiprobability*, π, be defined as

$$\pi = \left[\frac{p - q}{1 - q}\right],$$

in which the ' square ' brackets indicate that $\pi = 0$ if $q > p$. The quasiprobability reduces to p when $q = 0$. We know from T17 that the quasiprobabilities are multiplicative for a chain, and the strength of the chain is the same as if the quasiprobabilities were ordinary probabilities and the q's were all zero. Also, from T15, we have

$$S(p, q) = -\log (1 - \pi),$$

so that for a bundle of the type occurring in T10 the quasiprobabilities again behave like probabilities, in view of the additivity of the strengths of the chains.[1]

Let us now consider an arbitrary finite causal net having independence and leading from F to E. We should like a general procedure for defining the strength of such a net that will include the results for the nets already considered, and which is simple, and which does not lead to a contradiction. I believe that the procedure illustrated in the following example satisfies these conditions. It would of course be more satisfactory if some convincing axioms could be laid down that would uniquely determine the procedure.

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[1] The term ' pseudoprobabilities ' would conveniently refer (by analogy with the pseudo-random numbers that are often used in Monte Carlo calculations) to the apparent probabilities that occur in a deterministic, but pseudo-indeterministic, set-up.

In the diagram, the quasiprobabilities $\pi_1, \pi_2, \ldots, \pi_8$ are assigned, and pertain to the links of the net. It will be easier to appreciate the example if the π's are at first thought of as ordinary probabilities (with all the q's equal to 0).

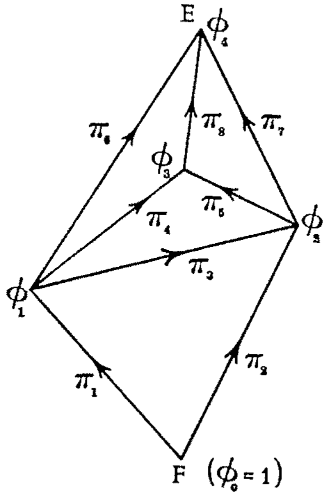

FIG. 2

The ϕ's may be thought of as quasiprobabilities of the *events*. They are defined successively as follows :

$$\phi_0 = 1. \quad \phi_1 = \pi_1.$$
$$\phi_2 = 1 - (1 - \pi_2)(1 - \phi_1\pi_3).$$
$$\phi_3 = 1 - (1 - \phi_1\pi_4)(1 - \phi_2\pi_5).$$
$$\phi_4 = 1 - (1 - \phi_1\pi_6)(1 - \phi_2\pi_7)(1 - \phi_3\pi_8).$$
$$S(n) = Q(\phi_4, 0) = -\log(1 - \phi_4).$$

The reader should perhaps check that this procedure contains the previous ones as special cases.

12 *Causal Nets in General*

It will often be possible to divide up a time-slice preceding E into non-overlapping events whose causal influences on E are approximately but not absolutely independent. Let such a dissection of the time-slice be $F_1, F_2, \ldots, F_m$. We need a definition of the strength of the causal link $F_1 \rightarrow E$ that will reduce to the value given previously in the case where F_1 and $F_2 . F_3 . \ldots F_m$ are *causally independent* with respect to E, in the same sense as that defined above for nets having independence. A simple definition having the required property is

$$S(E : F_1) = \log \frac{P(\bar{E} \mid \bar{F}_1 . F_2 . \ldots F_m)}{P(\bar{E} \mid F_1 . F_2 . \ldots F_m)} = W(\bar{F}_1 : \bar{E} \mid F_2 . F_3 . \ldots F_m).$$

This definition reduces to the previous use of the expression $S(E : F)$ in the case of causal independence. But the strengths of the lead-ins do not add up to $S(E : F_1 . \ldots F_m)$ unless the F's do have independent causal influences on E. We can cope with this difficulty by the introduction of ' interaction terms ' in a sense analogous to the use of this expression in the literature of the design of statistical experiments.[1]

We can think of an extra node in the causal net leading to E corresponding to every subset of the events $F_1, F_2, \ldots, F_m$. For example, there will be a node corresponding to the pair $(F_1 . F_2)$. The strength of the link from the node $(F_1 . F_2)$ to E will then be taken as the ' interaction ' term

$$t_{12} = s_{12} - s_1 - s_2,$$

where

$$\begin{aligned} s_{12} &= \log \frac{P(\bar{E} \mid \overline{F_1 . F_2} . \ldots F_m)}{P(\bar{E} \mid F_1 . F_2 . \ldots F_m)} \\ &= W(\overline{F_1 . F_2} : \bar{E} \mid F_3 \ldots F_m). \end{aligned}$$

When F_1 and F_2 are independent causes of E, we have $s_{12} = s_1 + s_2$, and the second order interaction term vanishes. The strength of the link *to* $(F_1 . F_2)$, from an earlier event, G, is

$$W(\bar{G} : \overline{F_1 . F_2} \mid G_1 . G_2 . \ldots),$$

where $G_1, G_2, \ldots$ are the other immediate predecessors of F_1 and F_2. The definitions of the *s*'s are forced, if we regard conjunctions of the F's as single events. An example of a third-order interaction is

$$t_{1234} = s_{1234} - s_{234} - s_{134} - s_{124} - s_{123} + s_{12} + s_{13} + \ldots + s_{34} - s_1 - s_2 - s_3 - s_4,$$

where the notation is now self-explanatory. In any piece of causal analysis one would try to choose the dissection of the time-slice so as to make the high-order interactions negligible.

Since

$$s_{123 \cdots m} = \sum s_i + \sum t_{ij} + \sum t_{ijk} + \ldots,$$

our enlarged causal net has the property of additivity of strengths of lead-ins that we previously had for causally independent lead-ins. It is therefore now potentially possible to apply the method of Section 11 to define the causal strength of an arbitrary finite net from F to E.

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[1] See, for example, *Design and Analysis of Industrial Experiments*, ed. by O. L. Davies, London and Edinburgh 1954; index reference under ' Interaction '.

13 *Degrees of Causation*

We may now define $\chi(E : F)$ as the limit of the strength of the net joining F to E and containing all intermediate events, when the events are made smaller and smaller. I have not proved that this limit exists. The proof, if possible, would depend on a physical theory, and would be mathematically intricate. Note the implication : whether degrees of causality exist is a matter of physics, even if we take for granted that physical probabilities exist.

In practice one must always over-simplify or simplify in order to be able to judge, estimate, or guess, the value of $\chi(E : F)$. (In the past, χ has been given only a few values, such as 'small', 'moderate', and 'large'.) There is always the possibility that something has been overlooked. Even in a statistical experiment involving randomisation, from which we can apparently deduce that some $\chi(E : F)$ is large, in fact E and F may both have been caused by some preceding event. The table of random numbers might have been seen by the famous lady tea-taster,[1] or there may have been some psychokinesis. We are always thrown back on judgment.

14 *Big Events*

So far the analysis has assumed F and E to be small events. If F is big we may imagine it split up into many small events, and imagine all these to be 'short-circuited' from an earlier 'input node'. By 'short-circuited' is meant that the resistances of all the imaginary links are taken to be zero. We may apply a similar process to a big E by short-circuiting its small parts to a future output node. The previous methods may then be applied even if F does not end before E begins.

Appendix I. Correction of some errors in previous work

Reichenbach[2] says that F is causally relevant to E if $P(E \mid F) > P(E)$ and if there is no set of events earlier or simultaneous with F that 'screens off' E from F. By 'screens off' he means that the probability of E given these other events is unchanged if F is also given. The property is analogous to the Markov property.

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[1] Sir Ronald A. Fisher, *The Design of Experiments*, 5th edn., Edinburgh and London 1949, Chapter 2

[2] Page 204 of the first reference in footnote 1, p. 45 above

It seems to me that this definition is not acceptable as it stands for much the same reason that my previous paper is not acceptable. For let G be any set of events earlier than or simultaneous with F. G might be some exceedingly biased selection of individual molecules, such as those that are proceeding south at a thousand miles per hour. Consider the expression $P(E \mid G) - P(E \mid G . F)$. Normally this will be positive for some G, say G_1, and negative for some G, say G_2. We now imagine G_1 to be gradually distorted into G_2. The above expression must change sign at some point during this gradual distortion, at which ' time ' its value will be zero. Hence the second part of Reichenbach's definition seems to be vacuous. In order to patch up the definition it seems to be necessary to take G as the complete state of the universe at the time F started.

In my previous paper, conditions C7 to C10 were vacuous for much the same reason, though it may be possible to patch the thing up, as stated therin (inserted in proof), by insisting that G should be in some sense a ' natural ' event.

Appendix II. The meaning of ' state ' in quantum mechanics (see Section 4)

The seven relevant interpretations of ' state ' in quantum mechanics are the first seven on the following list. All seven of these meanings, and perhaps others, should be taken into account in a comprehensive discussion of the place of probabilistic causality in quantum mechanics.

(*i*) The class of all past phenomena, classically describable. (*ii*) The class of phenomena extending only a short way into the past. (*iii*) The wave function of a physical system, under observation by another physical system. (*iv*) The joint wave function of the pair of systems. (*v*) The wave function of one system conditional on an assumed wave function of another system. This is the ' relative state ' of Hugh Everett III, ' Relative state formulation of quantum mechanics ', *Rev. Modern Physics*, 1957, **29**, 454-462. (*vi*) The wave function of the entire universe if this has any meaning. See Everett, *loc. cit.* (*vii*) The wave function of the entire universe together with all other past phenomena. (*viii*) An ensemble of wave functions. See, for this eighth interpretation, R. C. Tolman, *The Principles of Statistical Mechanics*, Oxford 1938, Section 98.

(*concluded*)

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COMMUNICATIONAL EPISTEMOLOGY (II) *

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2 *Correction of Misunderstandings*

OUR inquiry may be divided into the following questions:

(1) How does A find out B's thinking pattern, encoding and decoding functions?

(2) How does A know that his interpretation of B's behaviour is correct? How does A check the correctness of his interpretation?

(3) How does A know that B's interpretation of A's behaviour is correct? How does A check the correctness of B's interpretation?

(4) How does A become aware of the incorrectness of his interpretation of another person's behaviour?

(5) How does A know that B has become aware of B's misinterpretation of A's behaviour?

(6) What do people do to remove the detected misunderstandings? Do they employ a successful method? Do they employ the best method?

(7) What are successful methods and the best methods for eliminating misunderstandings?

The first question is essentially what is known to communication engineers and behavioural scientists as the problem of the black box. A black box is a transducer (or a processing system) which processes a given input into an output different from the input. The processing operation inside the black box is unknown. It is not open for inspection. All one can do is to examine the relationships between various forms of the input and their related outputs. One has to infer the nature of the operation inside the box from these relationships. A classical example of the black box is the animal used in stimulus-response studies in psychology. Communication engineers and mathematicians have elaborated refined theories of the solution of the black box problem.

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* Part I appeared in the previous Number.

A person as an epistemological object is a black box both to other persons, and, to some extent, to himself because much of his internal thought processes and ' state algebra ' is at the level of the unconscious. For this reason, communicational epistemology should include the study of the internal communication of the individual, though here we limit ourselves to discussion of interpersonal communication. Any other person, especially a new acquaintance or some one from a foreign culture, is more or less a black box. He is a triple transducer, in which three transducers are connected in series—the decoder, stal-operator and the encoder. The output of the decoder does not come out of the black box, but goes directly to the input of the stal-operator. Likewise the output of the stal-operator becomes the input of the encoder, and only the output of the encoder comes out of the black box. Another complication is that the person learns and unlearns, i.e. changes his stal, encoding and decoding functions, and so the properties of the black box transducers are not constant. And so Question 1 is a very complex black box problem. But, as long as it is a black box problem, it is possible to formulate it mathematically. Some existing theories partly solve the problem. The problem of a stal without encoder and decoder has already been solved except for some initial states.[1]

For reasons to be given later, there is no way of affirming the correctness of one's interpretation of others. There is, however, a way of detecting an incorrect interpretation. It is provided by the [illegible]iterion of inconsistency, not of inconsistency between one's inter[illegible]tation and another person's statement or action, but of inconsistency [illegible]*in* one's interpretation. One has no access to another person's statement or action except through one's decoding. One may ask another person whether one's interpretation is correct. But the question has to be decoded by this other person, and the answer is first coded by this other person and then decoded by oneself. Anthropologists often find that the questions put to persons within a culture about the culture are often irrelevant or meaningless to the persons asked, not for linguistic or semantic reasons, but because of cultural differences; and the answers, if answers are insisted upon, are meaningless or do not answer the question.

One becomes aware of the incorrectness of one's interpretations when one discovers inconsistencies within them. But it may happen that the correct interpretation seems strange or inconsistent to the interpreter because he is not used to interpretation of that type. In such a case, he at least discovers that there are differences between his own thinking pattern and that of the other. For example, should a Chinese philosopher say: ‘In Chinese philosophy the concept of substance does not exist, and the subject-predicate relationship is dispensable’,[1] the interpretation may be correct according to the Chinese pattern of thinking, but this correct interpretation is inacceptable to most Western philosophers who base their logic on the concept of substance and the subject-predicate relationship. This point will be further discussed under ‘dimension reduction’ and ‘projection’. A further example is provided by the important rôle played by the concept of the unconscious in psychoanalytic and psychiatric thinking. For many mathematicians and physicists the concept of the unconscious is unacceptable and is inconsistent with their thinking. Thus, an inconsistency within one’s interpretation of another person’s behaviour does not necessarily mean that the interpretation is incorrect. It means one or both of two things: (1) One’s interpretation is incorrect; (2) One has to change the thinking pattern in terms of which the interpretation appears inconsistent.

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[1] C. E. Shannon (editor), *Automata Studies*, see especially the article by Moore

Let us restate the preceding discussion more formally. Let the situation be that a person R interprets another person T. The transmitter of the communication, T, encodes his thoughts or state of mind into conscious or unconscious, verbal or nonverbal behaviour. The receiver, R, decodes T’s communication consciously or unconsciously[illegible]

Let f be the encoding-function of T, f_c and f_u the parts of f pertainin[illegible] to conscious and unconscious encoding respectively. Let T’s decodi[illegible] function be the inverse of f, denoted by f^{-1}. Let g, g_c, g_u be simil[illegible] the encoding functions of R, and g^{-1} the inverse of g. Let f, g, f^{-1}, g^{-1} be univalent functions. Let x represent the internal state of T and let x_i represent different values of x. (The subscript i need not be a denumerable ordinal nor even a member of an ordered set, but the axiom of choice has to be assumed.) It should be noted that $f_c(x)$ does not necessarily have to be decoded by g_c^{-1}: it may be decoded by g_u^{-1}. Similarly, $f_u(x)$ may be decoded by either g_u^{-1} or g_c^{-1}.

At the beginning of the interaction between T and R, nothing about f is known to R, and nothing about g is known to T. The functions f_c and g_c can be adjusted during the interaction by the process of approximation in order to give maximum understanding or maximum misunderstanding, depending on whether the purpose of the communication is understanding or deception, forming a non-zero sum two-person game of 'evolving' strategies—to be defined later. The function f_u cannot be adjusted consciously by T, and R has to learn to adjust g^{-1} to f_u by experience. Similarly g_u^{-1} cannot be adjusted by R consciously, and T has to learn to adjust f to g_u^{-1}. This one-sided adjustment is a non-zero sum one-person game of evolving strategies. The only criterion R may use for determining whether $g^{-1}f(x)$ is equal to x or not is the consistency or inconsistency of the set of $g^{-1}f(x)$ in the thinking pattern of R which we will denote by J. Symbolically,

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[1] T. S. Chang, ‘A Chinese Philosopher’s Theory of Knowledge’, *ETC.: A Review of General Semantics*, 1952, 9, 203-226

$$J\binom{S}{1}g^{-1}f(x_i)\in C$$

where $\binom{S}{1}$ is the set when x_i runs through all values of x, $\in$ is elementhood relationship, and C is the set of all consistent thinkings. If this is not satisfied, J and g can be gradually adjusted to give a maximum consistency:

$$\operatorname*{Max}_{m,n}\ \mathrm{Con}\, J_m\binom{S}{1}g_n^{-1}f(x_i)$$

The consistency criterion ensures that the interpretation of R is consistent, but not that it is correct. There may be self-consistent but mutually contradictory interpretations. Though R may succeed in constructing several different consistent interpretations he may not find the right one, i.e. the consistency criterion does not guarantee that $x_i = g_n^{-1}f(x_i)$. It does not help, in order to check whether R's interpretation is correct, to feed $g^{-1}f(x)$ directly back to T, because it will only give $f^{-1}gg^{-1}f(x)$, which is x, since f, g and their inverse functions are univalent. If a man thinks that π is an irrational number and says 'π is an irrational number' to a parrot, then if the parrot replies 'π is an irrational number' and the man 'understands' what the parrot says and agrees with it, it does not guarantee that the parrot's decoding of the man's message is the same as the man's. Similarly, a professor of philosophy may make a statement to a student. The student interprets the statement in his own way and expresses his interpretation by making exactly the same statement as the professor's. And the professor replies: 'That is right.' But this does not prove that the professor's thought and the student's interpretation of the professor's statement are the same. A better check is to make the student reformulate the statement in other words, or to make him apply it to different examples. The reformulation and the application of the statement to examples are intellectual operations. If we make a ‘ stal ’ complex enough, we can include such intellectual operations in the ‘ stal ’.

If the student reformulates the given statement in the same way as the professor would, or if he gives the same kinds of examples as the professor would, then the professor thinks that the probability that the student understood the statement is greater than otherwise. Similarly, we can make R process his interpretation and express the result of his processing. For example, if T transmits the word ‘ bird ’ by telephone and lets R spell out ‘ b . . . i . . . r . . . d ’; or transmits the numbers 3 and 5 and lets R add them and feed back the result; or transmits ‘ Your father does not approve your choice of career ’ and lets R figure out the solution and reply, then all these replies provide some check on R’s interpretation of T’s first statement. Symbolically,

$$f^{-1}g\, p_R\, g^{-1}f(x)$$

where P_R is the process by R, which may or may not be equal to P_T, where P_T is the process by T. If we find that it is equal for all P_R’s (and for corresponding P_T’s) and for all x_i, then for the practical purposes of life we may assume that $g^{-1}f(x)$ is equal to x, because on the behavioural level we cannot detect the differences between f and g. Here is a way of formulating behaviourism in terms of the mathematics of communicational epistemology. It should be emphasised, however, that communicational epistemology is not a form of behaviourism—a point which will be discussed at a later opportunity.

We can take a further step. Let the set of all P_R’s be U, and the set of all P_T’s be V. Then U and V are *isomorphic* if and only if the following conditions are satisfied:

(1) There is a one-to-one correspondence between U and V, i.e. for each P_R, $f^{-1}gP_Rg^{-1}f(x)$, which is P_T, is uniquely determined, and no two P_R’s correspond to a same P_T. And for each and every P_T there is at least one P_R corresponding to it.

(2) For any two arbitrary elements P_{Ri} and P_{Rj} of U, let

$$P_{Ti} = f^{-1}g\, P_{Ri}\, g^{-1}f,\ P_{Tj} = f^{-1}g\, P_{Rj}\, g^{-1}f$$

Then $$P_{Tj}\, P_{Ti}(x) = f^{-1}g\, P_{Rj}\, P_{Ri}\, g^{-1}f(x),$$

and similarly for higher combinations also. Note that the second condition follows from the first if the associativity holds between f, g, P_R, and P_T. But since associativity is not assumed, the second condition has to be stated explicitly.

The fact that U and V, are isomorphic does not guarantee that U and V, as sets, are the same. If they are, they are said to be *autoisomorphic*. The fact that U and V are autoisomorphic does not guarantee that each of their elements corresponds to itself. It may be that an element in U corresponds to a different element in V, even though U as a set coincides with V as a set. If each element corresponds to itself, the correspondence between U and V is said to be *element-autoisomorphic*. The concepts of isomorphism and autoisomorphism are discussed in further detail in group theory, ring theory, etc., in mathematics.

The criteria of isomorphism, autoisomorphism, and element-autoisomorphism correspond to increasingly close degrees of similarity between thinking patterns.

In classical epistemology, the individuals' cultural background was either abstract or homogeneous, and hence the encoding and decoding functions, especially their unconscious parts, are assumed to be the same for all individuals concerned. If a sincere will to communicate prevails f_c and g_c^{-1} are usually adjusted so that $g^{-1}f(x)$ will approach x. On the other hand, the adjustment of f_u is usually neglected or avoided by T because it is difficult to notice the characteristics of one's own attitude, behaviour, pattern of life, etc., and even if T notices them, it is difficult for him to change them. One of the advances of communicational epistemology over the classical intersubjective epistemology is the consideration of differences in f_u and f_u^{-1} for different individuals.

Let us now return to the theory of evolving games. In the traditional theory of games, the strategies and their *a priori* probabilities are known and only their actual *a posteriori* choice is an *a priori* unknown. Further, the information, when available, was ' objective ' information which did not go through the decoding function different from person to person. (The subjective non-linear evaluation of payment was discussed in some aspects of game theory, but this is different from the decoding of information.) The process of interpersonal communication is a series of tactical moves performed by participants, whose purpose is to increase mutual understanding or to increase reciprocal or unilateral misunderstanding (in deceptive cases). Consequently interpersonal communication is a non-zero sum game. The present theory of games differs from the traditional theory in two respects.

First, the strategies are neither chosen prior to the beginning of the play, nor are they distributed with probabilities; at the beginning of play, a player knows neither his own strategies nor those of the other players, but learns his own as well as the others' strategies as the play proceeds. Secondly, the information available is not 'objective'; it is coded by the transmitter and decoded by the receiver. At the beginning of play, a player knows neither the encoding and decoding functions of the other players, nor the unconscious parts of his own. Furthermore, even much of the conscious part of his own coding and decoding functions, because it is so natural, basic and automatic, escapes his explicit understanding. One does not realize *how* he thinks, encodes and decodes, until he communicates with someone who does otherwise. Thus a player learns a great part of his own encoding and decoding functions as well as those of the other players as the game proceeds. Even if good will for mutual understanding prevails, a large part of a player's encoding and decoding functions remain unknown and incommunicable to other players.

Because this type of game involves learning of strategies, encoding and decoding functions during the play, we call this type an 'evolving game'.

It will be helpful now to introduce the terms *dimension reduction* and *projection*. Dimension reduction is a process by which the interpretation *reduces* the dimensionality of the phenomena interpreted, while projection is a process by which an interpretation *adds* dimensions to the phenomena interpreted. Whether the reduced or projected interpretation is consistent (i.e. without contradiction) within itself depends on the nature of the dimension reduction and projection. A few examples of dimension reduction follow.

Example 1. Consider the motion of a particle moving in a Cartesian co-ordinate system so that its co-ordinates at time t are $x = at$, $y = -t^2 + bt$. The path of the particle is the parabola $y = -(1/a^2)x^2 + (b/a)x$. This is the path of a particle moving in a field of constant force whose vector is $(0, -2m)$ where m is the mass of the particle. This situation is consistent with classical two-dimensional mechanics. The G-projection (using this term to distinguish geometrical projection from projection as defined above) of the motion on the x-axis from the point $(0, \infty)$ gives a one-dimensional motion, $x = at$. This is consistent for classical one-dimensional mechanics. The G-projection of the motion on the y-axis from the point $(\infty, 0)$ gives the one-dimensional motion $y = -t^2 + bt$. This also is consistent for classical one-dimensional mechanics in a field of constant force.

Example 2. Again consider a two-dimensional Cartesian coordinate system in which two circular particles P_1 and P_2 of equal mass and equal radius r are in motion, P_1 moving along the line $y = a$ with velocity v_x, P_2 moving along the line $y = a - \sqrt{2}r$ with velocity $-v_x$, in such a way that they collide. Let us assume that there is no loss of kinetic energy in the impact. After the collision P_1 continues its motion along the line $x = b$ with velocity v_y, where $|v_y| = |v_x|$, and P_2 continues its motion along the line $x = b + \sqrt{2}r$ with velocity $-v_y$. The whole situation conforms with classical mechanics. If we now G-project the motion perpendicularly to the x-axis, the law of conservation of energy is violated in the resulting picture. After the collision there is no motion. We have to rationalise the anomaly by assuming, for example, potential energy at the points $(b, 0)$ and $(b + \sqrt{2}r, 0)$. On the other hand, a G-projection perpendicular to the y-axis will result in the apparent creation of kinetic energy out of no motion. Again a rationalisation becomes necessary, by assuming, for example, potential energy at the points $(0, a)$ and $(0, a - \sqrt{2}r)$. But a G-projection perpendicular to the line $y = -x + c$ will save the law of conservation of energy.

Example 3. An agnostic has no religious dimension. A philosophical system which includes all his points of view plus a religious dimension may become, depending on the method of dimension reduction, a self-consistent interpretation into an agnostic philosophy.

From a formal point of view the agnostic philosophy is more general than the religious philosophy, because it is less specific. But from the point of view of content, the religious philosophy is more general because it embraces more. The interpretation of generality here depends on whether one considers the addition of an extra dimension as an increase of specificity or as an enlargement.

Example 4. A person from a culture with little interest in the taste of interior decoration visited another culture in which tastefulness was reflected even in the interior of railway trains. Lacking the dimension ‘ tastefulness in interior decoration ’, he exclaimed that the train was ‘ super-clean ’.

In contrast to dimension reduction, projection (to be distinguished from G-projection) adds, in the interpretation, extra dimensions which do not exist in the phenomena to be interpreted. Usually the dimensions projected are traits that the decoder himself possesses or traits that he found in other persons in his previous experience. Projection of traits found in others is, in popular terms, usually spoken of as ' generalisation ', but this would be misleading in the present context, and it is better to call it ' extrapolation '. Among the traits most frequently projected are fear, suspiciousness, aggressiveness, connotativity of communication, i.e. communication by hint and insinuation, etc. A suspicious person believes that other persons are also suspicious. An aggressive person expects every one else to be aggressive. A person who uses indirect insinuations reads non-existent meanings between the lines of another person's statement.

A Western philosopher may apply dimension reduction or projection in his interpretation of Chinese philosophy which has no concept of ' substance ' corresponding to that in Western philosophy and in which the subject-predicate relation is dispensable. He may insist that there is a word in Chinese which translates ' substance ' and that he can identify subject-predicate relationships in the syntax of Chinese. But, from the Chinese point of view, these would be artificial constructs. There would be a dimension reduction if the event-centredness of Chinese epistemology were not recognised. There would be a projection in the sense that the extra dimensions of substance and of subject-predicate relationships are added. It has been emphasised by B. L.Whorf[1] that what is decisive is not artificial possibilities of certain categories, concepts and relationships in a thought pattern, but their naturalness in it. (Whorf has often been misinterpreted to mean that the linguistic structure *determines* the thought pattern; he among several other writers meant only that the linguistic structure influences and in some cases limits the thought pattern.[2] I have elsewhere discussed the distinction between total denial and partial denial, a distinction always made in Chinese, often absent in English, and always absent in Japanese, as well as imprecise adverbially modifying in English.[3] I have also discussed differences in thought patterns between Swedish and Danish cultures in spite of close similarity of linguistic structures.[4]

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[1] B. L. Whorf, *Language, Thought and Reality*, 1956, pp. 59, 63, 64, 145, 146, 157, 213, 265, 266

[2] *Memoirs of American Anthropological Association*, 1954, 79; L. S. Feuer, ' Sociological Aspects of Relation between Language and Philosophy ', *Philosophy of Science*, 1953, **20**, 85-100

[3] M. Maruyama, *Epistemology of Intercultural Understanding*, pp. 147, 148, 151

[4] Ibid., pp. 75-86, 99-101, 136-144

Thus, by dimension reduction and projection, a Western philosopher may construct a Chinese epistemology with the concept of substance and with the subject-predicate relationship, which is consistent not only in itself but also with Western philosophy.

We may also remark here that superstition and suspicion, which are variations of projection, are usually self-consistent. They are also self-proving in a circular reasoning. The belief in a supernatural power (an *a priori* assumption) is ' proved ' by interpreting, in terms of this *a priori* assumption, certain events to have been caused by a supernatural power. The hostility of a person (an *a priori* assumption) is ' proved ' by interpreting, by virtue of this *a priori* assumption, his behaviour to be indicative of his hostility. The ' logical ' proof of the concept of substance may be due to the fact that our logic is based on that concept. A philosophy without the concept of substance may as well be self-consistent. The development of logic and mathematical logic in the twentieth century has made it clear theoretically that, even within the deductive framework, it is possible to construct theories, each of which is consistent within itself, but which are incompatible with one another. This theoretical insight is not always appreciated when we reject certain patterns of thinking as ' illogical ' or ' unphilosophical '. In fact, such rejections often have ' illogical ' bases.[1]

We have seen that inconsistency in interpretation indicates one or both of two possibilities: (1) That the interpretation is incorrect, (2) That the interpreter has to change his thinking pattern within which his interpretation appears to be inconsistent. We have also seen that self-consistency of interpretation does not guarantee the correctness of the interpretation. We have discussed some methods of checking for correctness for practical purposes, for example by asking the receiver of the message to process the decoded message and feed it back. We also touched upon the concepts of isomorphism, auto-isomorphism and element-isomorphism of the thinking processes. As for the correctness of another person's interpretation, one has to interpret it in one's own pattern of thinking, and thus the problem becomes that of the correctness of one's own interpretation of others.

There remain yet some questions to be answered: What methods do people use to remove the detected misunderstandings? Are the methods successful? Do they use the best methods? What are the successful and the best methods? The answer to the last question has

[1] L. S. Feuer, ' The Bearing of Psychoanalysis upon Philosophy ', *Philosophy and Phenomenological Research*, 1959, 19, 323-340

not been worked out. It needs further studies. The answers to the first three questions are often discouraging. People may accuse each other of being illogical, dishonest, superficial, etc., or bury the misunderstandings under ' politeness ', calling this action ' tolerance ' or ' considerateness ' or even ' understanding '; they may reduce the communication to a practical business level, or may even disrupt communication entirely. In addition to logical factors, psychological factors such as social perception, fear of loss of prestige through failure to get one's values appreciated, ' sour grapes ', rationalisation, filtering of perception and memory for reduction of cognitive dissonance, transference, instrumentalising perception, etc., have already been discussed elsewhere.[1] Let us now consider the third stage of our study—the limitations of interpersonal understanding.

(*to be concluded*)

[1] See Part I of this paper, second reference.

DISCUSSIONS

THE PARADOX OF CONFIRMATION (II)

THIS note is a continuation of a previous one[1] and consists of a correction and an addition to it. The correction is a serious one, and I must apologise for my previous error; but, as extenuating circumstances, I think I was right in applying semi-quantitative methods to the problem. Also I still believe that the idea of a Statistician's Stooge is useful.

Hempel's paradox is exemplified thus:

The hypothesis, H, that all crows are black is the same as that all non-black things are non-crows, and this is supported by the observation of a white shoe.

The kernel of my argument was to consider a 2 by 2 contingency table with its rows labelled 'crow' and 'shoe', and its columns 'black' and 'white', and then to find an algebraic expression for the weight of evidence in favour of H in virtue of the evidence, E, of the observation of a white shoe. (The use of the Stooge allows us to restrict our attention to 2 by 2 contingency tables.)

If the marginal totals of this table are known in advance, then E really does support H. But I stated in Part I that this conclusion follows even if the marginal totals are not known in advance, but only have probability distributions. Here I made a mistake.

In order to see this, let us suppose that we know in advance the total number of black shoes and the total number of white shoes that may be observed, each shoe being equally likely to be observed. Then it is a very simple matter to prove that the weight of evidence in favour of H provided by E is zero, where 'weight of evidence' is defined, for example, in Part I.

Thus a 'case of a hypothesis' does not necessarily support the hypothesis. (The example of a white raven shows that a case of a hypothesis can undermine it, but this example is irrelevant to Stooge-type experiments.)

The question arises, what makes us imagine that a case of a hypothesis must support it? Light is shed on this question by supposing an object to come gradually into view. Suppose that at some moment the object is seen to be white (event F). Then $P(E \mid F . H) = 1$, where E is the proposition that the object is a non-crow. But $P(E \mid F . \bar{H}) < 1$. Hence $W(H : E \mid F) > 0$. For example, the observation of a white shoe supports H when the object is already known to be white. But $W(H : F) < 0$ under a wide class of assumptions, including those where one knows in advance the physical probability that the first object observed will be a black crow. (For on this assumption, the probability of seeing a white object is a little smaller given H than given $\bar{H}$. Thus it may easily happen that $W(H : E . F)$, which is equal to $W(H : F) + W(H : E \mid F)$, may vanish although $W(H : E \mid F) > 0$.

Let us express the matter more generally. If we wish to test a hypothesis of the form that all A's are B's, we may go on sampling objects until we find an A, and then look to see if it is a B, the observation being of Stoogian type. If it is a B, then the hypothesis is to some extent supported *if the evidence of how long it took to find an* A *is suppressed.*[1] More formally, the weight of evidence in favour of the hypothesis provided by B, *given* A, is positive. But what may be overlooked is that if A's are surprisingly easy to find, this may by itself undermine the hypothesis that all A's are B's. Thus the total evidence may not support the hypothesis after all.

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[1] I. J. Good, this *Journal*, 1960, 11, 145-149

This general thesis has already been exemplified above, with $A =$ non-black, $B =$ non-crow; but perhaps a further example will clarify the thesis further.

Suppose we are told that all men in Ealing whose surnames end with the letter z are escaped convicts. We take a random sample of the citizens of Ealing, and, after a very short time, we find one whose surname ends with z. Then the fact that we found such a one so quickly tends to undermine the hypothesis, for this evidence by itself suggests that there are more people whose surnames end with z than we had previously supposed.

Thus there is more than meets the eye in the expression ' a case of a hypothesis '. In one interpretation it necessarily supports the hypothesis, in another it need not do so.

Although it is not strictly necessary for our argument it seems desirable to elucidate the contention that ' all crows are black is the same as that all non-black things are non-crows ', especially as this contention was called into question at a recent London Symposium on information theory by D. M. MacKay.[2] As propositions the two statements are logically equivalent. But they have a different psychological effect on the experimenter. If he is asked to test whether all crows are black he will look for a crow and then decide whether it is black. But if he is asked to test whether all non-black things are non-crows he may look for a non-black object and then decide whether it is a crow. If he engages a Stooge to perform the second experiment, the Stooge may see several black crows before finding a non-black object, and say nothing about the crows in his report. This second method of performing the experiment would be very inefficient as compared with the first method, in the sense that the expected weight of evidence per unit of effort would be much less. An intellige[illegible] experimenter, even if he is asked to test the hypothesis that all non-black things are non-crows, will set about looking for crows, or possibly ravens.

Afterthought. The following example shows that, in very special circumstances, the sight of a white shoe can actually undermine the hypothesis that all crows are black. Suppose that a child had seen black crows, black shoes, and no other black objects, and that all the crows and shoes had been black. He now sees a white shoe and he says, ' How surprising ! Apparently objects that are supposed to be black can sometimes be white instead.' On the information available to the child this may be a very rational thing for him to say.

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[1] The need for the observation to be of Stoogian type is exemplified by the white raven.

[2] In the discussion of the paper which I presented. The Proceedings of the Symposium are to be published by Butterworths.

COMMENTS ON DR NINIAN MARSHALL'S 'THEORY OF RESONANCE'

IN his paper 'E.S.P. and Memory: a Physical Theory'[1] Dr Ninian Marshall puts forward a set of assertions which he claims constitute a new theory in physics with applications to some problems in psychology, particularly the problem of the so-called Extrasensory Perception phenomena. The paper is an ambitious one containing a main thesis inviting comparison with Newton's formulation of classical Mechanics. (He formulates a 'law' on the explicit analogy of the Newtonian 'Law of Gravitation'.) Unfortunately when one examines his main thesis one finds a lack of precision which rules out any hope of Marshall's being the long sought 'Newton of the Mind'. Furthermore, as I shall show, there is conflict with empirical fact in connection with the only positive prediction which one can immediately draw from this admittedly 'qualitative' and imprecise, though highly pretentious, paper.

Put briefly, Dr Marshall's main thesis is this: there is a feature in the physical world which has hitherto escaped the notice of physicists, and other natural scientists, because its effects are too small to be detected in the realms with which they are concerned. This feature which the author calls 'Resonance' is said to obtain significantly only between 'complex structures' manifestating 'patterns' which have some sort of 'similarity'. When 'resonance' occurs in a significant degree it is said to be responsible for a wide range of effects, in particular some of those ostensible occurrences called ESP. 'Resonance' is said to obey 'laws' which are 'formally' analogous to two of the laws of Newtonian Mechanics, namely the law of gravitation, and the law of force expressing the relation between acceleration and mass. Dr Marshall contends that on the basis of this theory he can make predictions which can be experimentally tested.

It is obvious that the strength of any such a theory, which claims to be scientific, must be determined by that of its basic tenets. If these bases cannot match the requirements of scientific method in the other sciences which it claims to complement, the theory (and any consequences entailed by it) becomes valueless. This I believe to be the position in regard to Dr Marshall's exposition of his theory, for the reasons that I shall now give.

Firstly, and fundamentally, the defect of the theory in contrast to Newtonian Mechanics is its complete failure to produce precise definitions of the basic concepts of 'complexity', 'pattern', 'similarity' and 'difference' between patterns. Yet these are key concepts in his theory and his so-called 'law of resonance' is expressed in terms of them. The law he states as follows:

> Any two structures exert an influence on each other which tends to make them become more alike. The strength of this influence increases with the product of their complexities and decreases with the difference between their patterns.

It will be obvious that the law in this form is useless for the purpose of making any precise predictions of the kind that could be tested. The author seems to think that new mathematical or scientific developments are required to make a more precise formulation possible. This is not so: there are well established methods which could perfectly well be used to make his concepts precise, and to make verified predictions from them, if his ideas are sound. For example: the mathematical theory of probability can be used to give precision to the concept of 'complexity' in regard to both structure and function. Indeed it has been so used for some time in the scientific discipline known as 'Information Theory'. The basic idea here is the notion of the antecedent probability of a particular configuration of elements in space (for example distribution of velocities of particles, arrangement of amino-acids along a polypeptide chain, etc.) or of processes in time (for example transitions between energy levels, simultaneous firing of neurones) arising by chance. The greater the antecedent *im*probability of the actually realised structure or process, the greater its 'information content' and 'information content' certainly corresponds very closely to what Dr Marshall appears to mean by 'complexity of pattern'. Thus the 'information content' of the neural processes underlying a conscious train of thought, such as a scientific discovery, is certainly greater than that of the 'ordering' process involved in the growth of inorganic crystals. Unfortunately the only prediction which seems possible on the basis of the imprecise formulation of the theory by its author, viz. that two almost identical cells should grow more, rather than less alike, is refuted by the known facts of embryology. I say more of this later.

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[1] This *Journal* 1960, 10, 268–286.

Again the notion of 'pattern' and 'similarity' and 'difference' of pattern, can be made precise by using the theory of relation-numbers set out in *Principia Mathematica* Vol. II, part IV, p. 150 ff. On Whitehead and Russell's theory we start from the notion of structures as consisting of ensembles of entities united by n-adic relations where n is any integer greater than one. Then two relations P, Q are said to be 'similar' if there is a one-one relation between the terms of their fields which is such that, whenever a pair of terms have the relation P their correlative terms, picked out by the one-one relation referred to, have the relation Q; and vice versa. Two relations which are similar in this sense are then said to have the same 'structure' or 'relation-number'; thus the 'relation-number' of a relation is the same as its 'structure', and is defined as the class of all relations similar to a given relation. Relation-numbers satisfy all the formal laws of arithmetic which are satisfied by transfinite ordinal numbers.

If therefore we interpret Dr Marshall's term 'pattern' to be equivalent to 'structure' as defined in *Principia Mathematica*, we can give further precision to his law of resonance quoted above; though, as I shall now show, there is still uncertainty about the application of the 'law' to his theory of telepathy and memory. In the *Principia* theory of 'structure' or 'pattern' a *necessary* condition for two ensembles of entities to be 'similar' in structure is that they should consist of the same cardinal number of *relata*, that is the fields of the constitutive relations must contain the same cardinal number of terms. But this condition is *not sufficient*. For example, the two ensembles consisting of the series of all natural numbers in order of increasing magnitude, and the series of all the odd numbers followed by all the even numbers, have exactly the same cardinal number of *relata*, that is $\aleph_0$ the cardinal number of the set of all the natural numbers, in each case. But the transfinite ordinal number of the former series is exactly half the transfinite ordinal of the latter. Therefore the two ensembles are NOT similar in structure and do not have the same 'pattern'.

The reason for this is of course that the set of all odd numbers has the transfinite cardinal $\aleph_0$ of terms, and so does the set of all even numbers. Therefore we can correlate one-to-one the set of the natural numbers to the set of all odd numbers; and then all the even numbers will remain without a correlate. We can make the two ensembles similar in structure by *replacing* the even numbers in the order in which they occur in the progression in order of magnitude of natural numbers. So the *necessary and sufficient* condition for two ensembles of entities to be *similar in structure* is that it must be logically possible for their terms to be correlated *without change of order*. We can now see that *dissimilarity* between patterns can take two forms:

(1) The two structures compared may *not* have the same cardinal number of related terms. We might call this Cardinal Dissimilarity to be measured by the difference in the cardinal number of the members of the two.

(2) The two structures compared may have the same *cardinal* number of terms but their *ordinal* or ' relation ' numbers may be different. In such cases, of what we may term ' ordinal dissimilarity ', we can measure the degree of dissimilarity by methods such as Kendall's 'Rank Correlation Coefficient' and his ' Coefficient of Concordance', (to deal with the case of sets of dimensionally independent rankings). In point of fact the easier thing to measure is ' degree of *similarity* ' by Kendall's technique. Such degree of similarity will in general take two forms:

(1) degree of direct correspondence between the ordinal numbers of two classes of characteristics, for example rankings in respect of skill in Music and Mathematics in a single school form, that is between two classes having the same *cardinal* number; this degree of correspondence can be measured in terms of the minimum number of moves which transform any ranking into any other ranking of the same collection of objects, for example boys of a given form in a school.

(2) Concordance between the communality of orderings of n objects (e.g. boys or geometrical points) in a manifold of m dimensions (e.g. different observers or m spatial dimensions). The degree of such concordance can be measured by Kendall's Coefficient of Concordance. (*Rank Correlation Methods* by M. G. Kendall, London, 1948, p. 81).

But this brings out the practical uselessness of Dr Marshall's concept of ' resonance ' once one gives it precision: how could one hope to establish with experimental techniques the exact *cardinal* number of neurones involved in a particular pattern of neural activity? Even more difficult would be the determination of the precise *ordinal* relationships. Yet both these kinds of data would be required to establish degrees of dissimilarity by reliable methods, for example by using Kendall's Rank Correlation Coefficient (which is a measure of the minimum number of changes of order required to transform or assimilate one structure to another, both structures having the same cardinal number of terms).

Finally I would like to point out that Dr Marshall's theory of Resonance seems to be contradicted by the facts of cell differentiation in embryology. The first stage of normal embryological development is the division of a fertilised ovum into two ' daughter cells ' which are practically identically similar in structure to each other and to the single parent cell from which they are derived. At the next stage of embryonic growth each of these two daughter cells produces two others yielding four in all. There is now slightly more difference between each member of the ensemble, but they are still practically identically similar in structure judged by current observational techniques. As the process of cell division and embryonic differentiation goes on we reach assemblies of cells, the members of which are more and more unalike in structure and function: till finally the full specialisation characteristic of the adult organism is reached. But it is clear that this process of differentiation and cell specialisation as we know it, is contrary to Dr Marshall's theory of Resonance, which entails that the two very closely similar cells, which are ' daughters ' of the original fertilised ovum, should grow *more* alike rather than give rise to more unlike ' daughter ' cells of their own. It is no use to say that the cells involved are not sufficiently ' complex ' structures in Dr Marshall's particular sense to serve as examples of the theory of resonance. For it is a fact that all the vast complexity of the hereditary structure of the human brain is contained in the genetic material of the original cell—the fertilised ovum, and the human brain is, according to Dr Marshall, the most complex structure in the universe.

So I conclude that Dr Marshall's theory of resonance is both theoretically unsatisfactory (because its formulation is so imprecise) and practically invalid (because such conclusions as one can draw from so imprecise a theory are in conflict with well-established empirical facts). It seems clear that ESP phenomena, if they do occur indeed, cannot be clarified by this kind of speculation. Nor can we expect Dr Marshall's theory to be of any help in the fundamental problems of Physics, Biology, and Psychology.

H. A. C. Dobbs

Reply to Dr H. A. C. Dobbs

Dr Dobbs's note on my article ' ESP and Memory: a Physical Theory ' offers three main criticisms of the theory of resonance—that it is imprecise, hard to apply, and in conflict with the facts of embryology. I regard one of these criticisms as true, one as over-simplified, and one as completely mistaken.

Dobbs is right to point out that the theory as presented there is imprecise. (I take it that he refers only to that article. For although he calls the imprecision a fundamental defect of the theory, he goes on to say that it could quite easily be remedied by established methods.) I was aware at the time that the basic concepts of ' complexity ' and ' similarity ' were not sharply defined. To publicise an as yet undeveloped theory is often not of immediate practical use, but may be of value in stimulating thought and discussion on a subject which is too big for the author to handle alone. I appreciate that Dobbs has contributed something in this way. I too shall not be satisfied with the theory until it is expressed more precisely. Indeed, to some extent this had already happened. There is a steadily growing family of possible measures of complexity and similarity, about which I hope to write more later. Here I shall confine myself to the material published so far.

Dobbs's suggestion that one can identify complexity with information content, and so with antecedent improbability, will not do. It is true that some simple structures (e.g. crystals) are more likely to occur that some complex ones, but it is not true in general. Let a line of n pennies show heads and tails at random. Then all the 2^n combinations are equally probable, but they are not all equally simple. (The simplest combinations show all heads, or alternating heads and tails, or the like.) The prior probability of an event depends on the context in which the event occurs, whereas its complexity is an intrinsic property; the two cannot therefore be equated.

A more adequate approach would have been as follows. Let a standard communication language be defined, containing various algebraic and logical symbols. Let the complexity of a pattern be defined as the length (in this language) of the shortest message needed to describe the pattern. If a pattern is repetitious (e.g. a wallpaper or a crowd) it will pay first to describe code symbols for the repeating units. The total length of the message will then be the length of the description of the code plus the length of the encoded message. Simple structures will tend to have short descriptions. This can be made as precise as one likes, and is one reasonable measure of complexity.

To define similarity is not so easy as Dobbs suggests. He invokes the mathematics of relation-numbers and of rank correlation coefficients, but he does not make clear how these are to be applied. There are several difficulties. His relation-number approach applies in its present form only to structures having equal numbers of identical elements. But structures may have similarities at different levels and of different kinds. For example, a mouse and an elephant have a similar anatomical structure. The grossest similarity is the one-one correspondence between the organs and their relationships in each animal. But the individual organs are themselves structures having varying degrees of similarity. At some levels there are differences not in the relationships between but in the number of elements in the two structures (e.g. a mouse liver has fewer cells than an elephant liver). How can this hierarchy of similarities be used to yield one scalar number, a measure of the degree of similarity of the whole? Dobbs does not say.

I am puzzled by Dobbs's suggestion that we can measure the relationships of points in space by rank correlation coefficients. Rank correlation coefficients measure ordinal similarities. Ordinal relationships are topological relationships in one dimension. How can they be applied to spatial relationships in three or four dimensions? If sets of objects (i.e. structures) are ranked in each of *m* spatial dimensions, as Dobbs suggests, then unfortunately the coefficient of concordance will depend on the initial choice of co-ordinate axes; it is not an objective measure. This idea may possibly bear fruit, but seems untenable in its present form.

Nevertheless, let us here assume (as I hope to show elsewhere) that a sensible measure of degree of dissimilarity can be given which depends only on the topological properties of the two structures. Two topologically identical but metrically different properties would, in this measure, have zero dissimilarity. But other measures could as well be defined which depended only on the projective, affine, or metrical properties of the two structures. Which type of measure should be used, if the theory of resonance is true, is another problem which has not yet been considered.

Dobbs claims that the theory of resonance is hard to use in psychology because so little is known about brain structure and function. This, of course, is an objection to every theory correlating brain activity with mental processes. With present techniques, no central neurophysiological process can be recorded in any detail. I did not claim that the theory would replace the study of neuroanatomy and neurophysiology; I claimed that it might be true. It should be tested on a complex artefact whose structure is known. In fact, this is feasible with existing electronic structures. If shown thus to be true, the theory would help the study of the brain in two ways. It would suggest some fruitful lines of research which would otherwise be ignored.

And it would provide a further method of recording brain activity—that is building a device which resonated with a living brain.

Finally, I do not agree that the theory of resonance is in any way contradicted by the facts of cell differentiation in biology. Let us suppose that at the two-celled stage an organism is completely symmetrical, but that asymmetry appears as it grows. This paradoxical process requires explanation whether or not the theory of resonance be true. The following explanation has been put forward by cyberneticists. Many structures have this property—for example a pin balanced on its point, or an infinity of electric circuits. These systems are initially symmetrical in structure, but soon acquire an asymmetry of function. For all their states of stable equilibrium are asymmetrical. They can acquire asymmetry of function in any of a symmetrical set of ways. (This fits the facts of equipotentiality of cells in early embryos.) If such a system is also a self-organising system, the asymmetry of function will then lead to the development of asymmetries of structure. Some such asymmetry producing mechanism must exist within a developing organism.

What difference would a resonance influence make to a system of this kind? It would often make very little difference. Resonances would be influences tending to keep the two cells similar, while some ordinary physio-chemical influences tended to make them become unlike each other. Which would win would depend on the relative strengths of the two sets of influences. The occurrence of differentiation would certainly not prove that no resonance influence had acted. Resonance was put forward, not as an all-embracing philosophical theory describing the overt behaviour of any two structures whatever, but as one influence at work among many. This was the point of comparing it to Newton's Law of Gravitation. (Gravitation does not imply that any two touching objects will remain in contact, nor that a meson cannot split.) Dobbs calls the present theory pretentious; to what does it pretend?

NINIAN MARSHALL

A POINT OF PROFESSOR DINGLE'S

REGARDING Professor Herbert Dingle's article 'The Doppler Effect and the Foundations of Physics II', I would like to comment as follows:

In the discussion of the hypothetical experiment of two observers A and Z, firing rockets in the same direction Z → A, Professor Dingle is in error. dV is not the velocity with respect to something, but the *change* in velocity of A or Z. A change in velocity can be measured, and even a change in relative position! It is the principle of today's inertial navigation, which is a very accurate method and which in no way contradicts the theory of relativity.

It is interesting to note that in the mean time this experiment is no longer hypothetical. It has been done, and it came out exactly as the physicists expected it would. Its result was communicated by H. E. Bömmel and K. Dransfeld at the Thanksgiving Meeting of the American Physical Society (25 Nov. 1960). A difference between the theory of relativity and a classical theory could not be detected in this experiment, since it would be a second order effect, proportional to $(dV/c)^2$. But Professor Dingle's discussion is only concerned with a first order effect. This can be derived either by a ballistic theory or the theory of relativity with identical results.

P. J. VAN HEERDEN

A REPLY TO DR VAN HEERDEN

I CANNOT, of course, in the absence of a description, comment on the experiment to which Dr van Heerden refers, but since he implies the relevance of a second order effect I must presume that, if it is actually my A/Z experiment, a spectrum displacement was observed by A or Z, and in that case an effect depending on motion has been observed in circumstances in which there is no *relative* motion. The effect must reveal absolute motion, and so violate Einstein's first postulate.

But I doubt whether the experiment is really equivalent to mine, for I was not concerned with what happened during the almost instantaneous firing of the rockets, but with a slightly later state of affairs. Suppose A and Z are a light-year apart, and the rocket-firing lasts a fraction of a second. What does Z observe a month later? (The times, of course, could be reduced for a terrestrial experiment, so long as they keep similar relative magnitudes.) If he observes a steady spectrum displacement from which a dV can be calculated, then that dV must on any theory, classical or relativistic, denote a 'velocity with respect to something'; it cannot be called a 'change' of a non-detectable velocity.

Dr van Heerden's remark that 'a change in relative position' can be measured suggests that he has not perfectly grasped the situation. Of course a change in *relative* position can be measured, and so can a change in *relative* velocity. But here, as I pointed out, the *relative* velocity of A and Z is always zero, so if there is a change of velocity it must be of *absolute* velocity.

I might add—though this is outside the subject of my paper—that if, in Bömmel and Dransfeld's experiment, an effect was observed corresponding to the momentary acceleration of the bodies, then, if those bodies had never been in *relative* motion, the general postulate of relativity was violated. In that case, if the special postulate still holds, something meaningless can meaningfully change. It was Einstein's conviction, on both epistemological and physical grounds, that this is inadmissible that led to his general theory of relativity and the consequent theory of gravitation.[1] I suspect, therefore, that if Bömmel and Dransfeld's experiment was really the equivalent of mine, the effect observed must have been due to the forces applied to the bodies, and not to the motion resulting from the application of those forces.

HERBERT DINGLE

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[1] *Ann. d. Phys.*, 1916, 49, 769

REVIEWS

Essays in the Philosophy of Science. By Charles S. Peirce.
Edited with an Introduction by Vincent Tomas.
The Liberal Arts Press, The American Heritage Series, No. 17, 1957.
Pp. xxii + 271. Paper edition, $1.00. Cloth edition $3.25.

Values in a Universe of Chance, Selected Writings by Charles S. Peirce. Edited with an Introduction and Notes by Philip P. Wiener.
Stanford University Press. London: Oxford University Press.
Pp. xxv + 446. 31s. 6d.

PROFESSOR TOMAS's collection of the essays, articles and lectures of Charles S. Peirce pertaining to the philosophy of science is a valuable introduction to the thought of this brilliant and difficult philosopher. By now, thanks to the appearance of several volumes attempting to explain the substance of Peirce's thought and his historical relationship to the pragmatists, a new interest has been generated in his work. It is probably very natural that much of this interest has been focussed on his connections with the pragmatists, i.e. James's too-generous claim of discipleship and the master's good-humoured and gentle disclaimer made a natural starting point for scholarly investigation. Even though the results of such studies have amply demonstrated Peirce's independence of what are usually considered to be pragmatist doctrines, they have had the disadvantage of introducing Peirce to the contemporary philosophical reader in a negative way. First the contrast with James must be established, and then the positive discussion of Peirce's own views may be begun. This has had the disadvantage of making Peirce appear a philosopher who had more original or more sophisticated or more powerful arguments to offer in answer to the same questions that James asked.

It is to be hoped that those who begin their study of Peirce by way of Professor Tomas's collection of writings in the philosophy of science will be prompted to ask some new and penetrating questions. Peirce was a philosopher in the great tradition: a man of science thoroughly at home in the mathematics, physics, chemistry and biology of his day and at the same time filled with a deep and critical understanding of his philosophical predecessors. He did not ask how it was that science was possible; he accepted its reality and was concerned to analyse rather how man, as scientist, conducted his inquiries into the real world. After a brief introduction the reader is presented with a collection of the most important of Peirce's writings in the philosophy of science (all originally published in the *Collected Papers of Charles Sanders Peirce*). Included are discussions of the nature of belief and inquiry, the logic of deduction and induction, the rôle of hypothesis in argument, a criticism of determinism, and arguments for the reality of natural laws. Professor Tomas has done us a great service in selecting these essays from the voluminous *Collected Papers* thus making these important writings easily available.

Professor Wiener's book is also a collection of Peirce's writings. While there is a slight overlap with the selections in Professor Tomas's book, at least three-quarters of the material included is completely independent. In addition Professor Wiener has the merit of presenting some important writings not available in the *Collected Papers*: the Lowell Lectures on the History of Science, Hume on the Laws of Nature, and Letters to Lady Welby being particularly interesting.

As its title would indicate, Professor Wiener's book is more concerned with the cultural and humanistic aspects of Peirce's philosophy. It is divided into sections each of which presents the relation of science to some humanistic concern, namely, science, materialism, and idealism; pragmaticism, a philosophy of science; lessons from the history of scientific thought; science and education; science and religion. Although the selections are uneven in importance, the total gives an attractive picture of Peirce as a wise and deeply humane man. For him there is no antithesis between science (and a philosophy that is primarily a philosophy of science), and values. Very important in his philosophy is the denial of the mechanistic, deterministic world of the positivists; his is a world of chance that allows full scope for all human aspirations. His tone is always one of rationality, liberalism, and good humour, completely devoid of the sentimental and the platitudinous.

NANCY SUTTON SABRA

The Idea of a Social Science. By Peter Winch.
Routledge & Kegan Paul, London, 1958. Pp. 143. 12s. 6d.

To what extent do, or (since there is little agreement on what they are) should, the methods of the social sciences approximate to those of the natural sciences? The problem is a hoary one and because it is both vague and (therefore?) difficult, debate about it rages still. Mr Winch's contribution, one of the newest, is unequivocally antinaturalistic. For *what Winch proposes to do is to subsume social science under ' philosophy ' as it is conceived of by disciples of the later Wittgenstein!* (Almost any formulation of this Linguistic Philosophy, as we may call it, is denounced as misleading. It might be described as the view that it is a very important task of philosophy to scrutinise the language of philosophers and seek out their misuses of ordinary language and over-burdening of terms. Such discussion of nuances of usage is thought to dissolve many traditional philosophical problems. E. Gellner, in *Words and Things*, provides a well-documented dossier on the school.) In support of this startling position Winch adduces what seem like, *prima facie* at least, new arguments.

First, unlike all other intellectual enquiries which *are* human activities, social science is also *about* human activities. It is the activity concerned with activities. Winch argues that human social behaviour is only intelligible as (or because it is) rule-following behaviour, and that the notion of what it is to follow a rule is fundamental to social science. But the analysis of following a rule is also fundamental in philosophy, according to those who agree with Wittgenstein's emphasis on language. We follow rules in every human activity, even natural science. Only social science, however, tries to be at the same time the science of rule-following while itself, *qua* activity, following rules. And, we are told, Wittgenstein's analysis of following a rule *a propos* of language is applicable to all rule-following activities, i.e. to all human activities, and is thus a fundamental insight of social 'science'. Following-rules-in-society replaces physical-law-obeying-behaviour in Winch's social science. But this analysis of Wittgenstein's shows rule-following to be crucially different from physical law-obeying.

> the notion of human society involves a scheme of concepts which is logically incompatible with the kinds of explanation offered in the natural sciences (p. 72). . . Whereas the man learns to understand the rule the dog just learns to react in a certain way . . . the concept of understanding is rooted in a social context in which the dog does not participate as does the man (p. 74).

Winch's second argument derives from his view of philosophy as 'the study of the nature of our understanding of reality' (p. 40). Philosophy of science, philosophy of religion, political philosophy, etc., are not to be understood as parasitic developments on the edges of science, religion, politics, etc. Rather are they attempts to find out what kind of understanding of reality is conveyed by science, religion, politics, etc. But there is a snag in the way of extending this view to the philosophy of the social sciences for:

> to understand the nature of social phenomena in general, to understand, that is, what is involved in a 'form of life', has been shown to be precisely the aim of epistemology (p. 42).

Thus social science *is* the philosophy of social science, which *is*, in turn, Linguistic Philosophy. This state of affairs comes about because the sociological concepts with which we try to render social phenomena intelligible are themselves part of social phenomena.

> The idea of war, for instance . . . provides the criterion of what is appropriate in the behaviour of the members of the conflicting societies. Because my country is at war there are certain things which I must and certain things which I must not do. My behaviour is governed by my concept of myself as a member of a belligerent community. The concept of war belongs *essentially* to my behaviour. But the concept of gravity does not belong essentially to the behaviour of a falling apple in the same way: it belongs rather to the physicists *explanation* of the apple's behaviour (pp. 127-128).

The sociological concept of war influences my behaviour in a way '*essentially*' different from the way the physical concept of gravity influences the falling of the apple (which is not at all). Also the sociologist cannot invent his own explanatory concepts but must work with concepts already used in our social thinking, speaking and acting. For these reasons sociological explanations can never be the same as physical explanations, so much so that Winch does not dub them 'explanations' at all but something more like 'attempts to render human behaviour intelligible'. The difference is, as we shall see, rather important.

Moreover, and finally, he argues that natural science is objective as between the phenomena it studies; its 'view of reality' requires it to be, to use the vogue word, 'ethically neutral'. Nothing, apparently, could be worse for the science of society for:

> a . . . sociologist of religion must himself have some religious feeling if he is to make sense of the religious movement he is studying and understand the considerations which govern the lives of its participants. A historian of art must have some aesthetic sense if he is to understand the problems confronting the artists of his period; and without this he will have left out of his account precisely what would have made it a history of *art*, as opposed to a rather puzzling external account of certain motions which certain people have been perceived to go through (p. 88).

Perhaps one's main objection to this book is that this last cited argument leads to the depressing conclusion that social science, like Linguistic Philosophy (which occasionally pretends to be proud of the fact), is impotent. For the very nature of human society, we are told,

> is to consist in different and competing ways of life, each offering a different account of the intelligibility of things. To take an uncommitted view of such competing concepts is peculiarly the task of philosophy. It is not its business to award prizes to science, religion or anything else. It is not its business to advocate any *Weltanschauung*. . . . In Wittgenstein's words 'Philosophy leaves everything as it was' (p. 103).

The rest of my comments will be lost on anyone who finds himself in sympathy with this attitude. If criticising and assessing *Weltanschauungen* is not the business of philosophy then I fail to to see whose business it is. I like this sort of activity and when I am doing it I like to think I am doing philosophy; the ' business ' Winch wants me to substitute, i.e. ' rendering intelligible ' would soon put me to sleep. Problems of intelligibility, communication, ' mental cramps ' and so on, are ' the rubbish that lies in the way to knowledge ' which we employ an under-labourer to sweep away before we get down to real discussion.[1] For we are interested in pursuing truth, not sweeping away rubbish. We prefer to concentrate on first order problems which it is our task to *solve*. We try to *explain* the social world. ' Understanding ' is involved in explanation, certainly, but there is more to it than that: explanation is both more concrete and more objective; which is to say we know what an explanation looks like, and we can usually agree on whether an explanation is satisfactory or not. None of this can we do in the case of ' understanding ' and ' rendering intelligible ', which both seem to be primarily psychological events.

Winch's arguments about what it is to follow a rule and that the concept of war influences our behaviour, seem to me to be a mere sophisticated re-vamping of the well-known 'oedipus effect' and 'intuitive insight' arguments. But there is one new point. We are told that we must have a religious feeling in order to understand a religious movement. (Do we need it to *explain* a religious movement?) We at once wonder whether this is any more true than the manifestly absurd idea that we have to empathise with an atom in order to understand its behaviour. ' Ah ', Winch could reply, ' you are already begging the question; my whole point is that there is an *essential* difference between human behaviour and the behaviour of physical objects. The concept of war belongs essentially to human behaviour.'

What Winch means is that the idea of war influences human behaviour: it is one of the factors in the situation of the individual. While the falling apple has no situation in that sense, and is not influenced by the concept (only by the force) of gravity. But in as much as the force of gravity can be said to dispose the apple to behave in a law-like fashion, i.e. ' falling '; so can the concept of war be said to dispose human beings to act in a law-like fashion. Both ' gravity ' and ' war ' are dispositionals: the only difference being one of degree, not kind: a human being is disposed, *qua* physical object, to respond to all the forces that the apple responds to; but in addition a person has the power, *qua* decision-taking, free-willed individual, to counter-act some of these forces, and is burdened with the nonphysical influences of other people, motives, concepts, theories, compulsions or whatever. To explain why that boy fell when he let go of the branch we need no more than we need to explain the fall of the apple. But explanations of why the boy was in the tree and why he let go of the branch would be very different from explanations of why the apple was on the tree and why it ' let go '.

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[1] There is a contradiction in Winch's book: a good deal of chapter 1 is spent denouncing the under-labourer conception of philosophy. Unfortunately the import of the book is not only to endorse Wittgenstein's under-labourer-ism (' rendering intelligible ' etc., etc.), but even to recommend that sociologists become this kind of under-labourer too.

To sum up. Gellner, an outsider to Linguistic Philosophy, first drew attention in *Words and Things* to the very strong sociological strands in Wittgenstein's later thought: in particular his view that words—read ' concepts '—are intelligible only in their particular sociolinguistic context. Winch, an insider, wishes to invert Gellner's thesis that philosophy is poaching on (poor) sociology, in an attempt to convince us that sociology, rightly considered, is the same as Linguistic Philosophy. But if, as may be the case, Gellner has demolished Linguistic Philosophy in that same book, then the final prop under Winch's case for a more *a priori* sociology is pulled away.

I. C. JARVIE

The Logic of Social Enquiry. By Quentin Gibson.
Routledge & Kegan Paul, London, 1960. Pp. x + 214. 24s.

MR GIBSON's main problem is the relation between the methods of the natural and the social sciences. In his introduction he rejects antinaturalism. But, although he is antinaturalistic, Mr Gibson is not unequivocally pronaturalistic. As a consequence, the book has an odd structure. Its first part is devoted to answering the objections of the antinaturalists; its second part discusses the ' logical peculiarities ' of social enquiry.

The *objections* to social ' science ' dealt with are: that it is too abstract; that it cannot formulate general laws; that it must use intuitive insight; that it is value-loaded; and that it is nonobjective. Nothing unusual here, you might say, and you would be right. However, it is clearly and succinctly done; although a good deal of it has already been done still better by Popper in *The Poverty of Historicism*.

The *logical peculiarities* of social science which Mr Gibson admits are these. All alleged social facts, and all alleged social laws, are, in the last resort, themselves explicable in terms of the aims and dispositions of individuals and, although this has not been carried out in every case, there seems no reason to believe that it could not be. No hard-and-fast line can be drawn between psychology and sociology: either psychology is equivalent to ' human nature ' and therefore includes sociology, or it is the narrow subject taught in universities in which case it is yet another social science for the social environment is one of the factors it must take into account. The use of law and tendency statements in social science is restricted by the very nature of the subject-matter; this is to some extent compensated for by the fact that human beings (unlike atoms) act rationally very often, which simplifies the task of explaining their behaviour. The logic of social enquiry closely resembles that of historical enquiry except that historians are interested in the statements of initial conditions and social scientists in the theoretical statements. The book concludes with a chapter pointing out that the implimentation of reform based on sociological knowledge is hindered by the necessity of gaining power; an aim which tends to make people lose sight of their original reformist aim.

I must confess to not knowing in which sense any of Mr Gibson's ' logical peculiarities ' are either logical or peculiar. Whatever he chooses to label them, though, does not affect one's agreement.

Where do the faults, if any, of the book lie? Principally, I should say, in its inductivism. Not that I now want to rehearse ancient objections to inductivism; although it is about time that people rejected Hume's extremely unsatisfactory solution to the problem, especially as it commits us to a form of irrationalism. More interesting is the mistake Gibson makes which is directly consequent on his inductivism. Believing that we learn by induction from experience Gibson has to admit that questions about the origins of our beliefs may be relevant to their truth. That is to say, one way of checking the truth of our beliefs is to see whether they were arrived at in a sound way. If they are so arrived at then they are ' rational ' beliefs in a way in which beliefs about walking under ladders are nonrational (to call them irrational prejudges the issue). Unfortunately I cannot see the difference between these two types of belief. As far as I can see Einstein's theory may have been a hunch of his in exactly the same way that the superstition about walking under ladders was originally someone's hunch. The *theories* are neither rational nor non-rational: they are nothing; the categories do not apply. It is the *belief in* the theories that can be so judged; but even here their origin is and must be irrelevant. It is more rational to believe in some of our hunches rather than in others simply because some of our hunches are better tested than others. Testing, clearly, has nothing to do with origins.

Gibson's view means he must condemn the mythologies and beliefs of primitive tribes as nonrational; while my view both distinguishes our beliefs from those of primitive tribes by pointing out that they are better tested and that therefore our *belief in* them is better founded than their belief in theirs is, and at the same time staves off that total relativism towards beliefs which anthropologists and linguistic philosophers alike have found to be the only resort after giving up the position Gibson takes.

My final criticism is of the book's style. Not, of course, of its literary style, which is clear and easy to read. Rather its exposition or, if you prefer, its method. The book has a dearth of illustration, not just of social laws, which may be difficult to come across, but of problems. The book exists in a vacuum, as it were; no attempt is made to let its argument stem from the treatment of a specific problem or set of problems as have most of the best treatises on the methods of science.[1] Mr Gibson wanted, or his publishers asked him, to write a book on the logic of social enquiry; as a consequence the book is, in the bad sense (is there any other sense?), a *text* book and, like most textbooks, rather dull.

I. C. Jarvie

Critical Problems in the History of Science. Edited by Marshall Clagett. University of Wisconsin Press, Madison, 1959. Pp. xiv + 555. $5.

This volume contains the Proceedings of the Institute for the History of Science held at the University of Wisconsin in September 1957, the primary purpose being to stimulate the study of the history of science. The names of the contributors make up an impressive list, and most of the papers give clear evidence of the tremendous advances in scholarship and insight that have taken place in this field during the past twenty-five years or so. All the major branches of the physical and biological sciences are represented, and the time-scale covered reaches from the Greeks to the nineteenth century. Papers like those of Professor Derek Price's ' Contra-Copernicus ', being ' a critical re-estimation of the mathematical planetary theory of Ptolemy, Copernicus, and Kepler ', or Professor E. J. Dijksterhuis's balanced survey of the origins of classical mechanics from Aristotle to Newton, will cause many old-fashioned estimates of these crucial periods to be revised. Professor Thomas Kuhn's study on ' Energy Conservation as an Example of Simultaneous Discovery ' brings together an immense amount of information and is already recognised as a minor classic in its field, whilst Professor I. Bernard Cohen's ' Conservation and the Concept of Electric Charge ' brings to life a subject too often ignored in the standard histories of science. And these are only samples.

For the readers of this journal of course the predominant interest will be philosophical; and it is of interest to note that throughout this volume the relevance of philosophical questions for an up-to-date treatment of historical problems is not lost sight of; indeed two or three papers make this the central issue. Needless to say, the contributors do not speak with one voice. Some, like A. C. Crombie, believe that ' a philosophical interpretation of the nature of modern scientific method and thinking enters into our historical interpretation of the course of events ' (p. 80). But whilst for Crombie this is of importance here only because he claims that classical mechanics and optics in part at least had their origin in reflections of the medieval methodologists on the nature of scientific knowledge, Father Joseph Clark believes that the researches of the historian themselves must be carried on not by applying chronological criteria but rather by taking into consideration 'the structure of a logically and systematically relevant pattern of central ideas' belonging to the field of philosophy of science (p. 104). Father Clark thereafter uses his conclusions concerning the logical structure of scientific theory, of the function of mathematics in science, and of the question of the correlation between abstract theory and experimental fact, to illuminate certain historical aspects such as the theory of Copernicus, the mathematical work of Oresme, and Beeckman's and Descartes's theories of free-fall. Apart from the question of the feasibility of this method for the historian, all this of course assumes an extant body of contemporary agreement concerning the nature of scientific theory; an agreement which Professor Ernest Nagel's incisive critical comments are quick to expose as being open to doubt. For Father Clark's views on the 'hypothetico-deductive method', the assumed free and arbitrary assumption of its postulates, and the putative 'isomorphism' between the formal structure and the physical world, are all matters open to debate. In other words, doubtful philosophy has to be used to estimate the relevance of perplexing historical problems.

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[1] Just one example from social science: Malinowski's functionalism grew out of his grapplings with the problem of the *kula*—or so I conjecture. Only later did he expound the doctrine *in abstracto*.

Still, nothing better could perhaps be expected; rather use provisional philosophical conclusions than none at all. But the matter is even more complex when we find that other contributors plead for the opposing (or is it complementary?) contention that no understanding of the philosophical issues of science will be possible without a grasp of its historical background. Thus, for Dijksterhuis 'the History of Science forms not only the memory of science, but also its epistemological laboratory' (p. 182). To give an instance from Professor Cohen's paper: for an understanding of the place of abstract philosophical maxims such as the causal principle in the body of a scientific theory, it is at least relevant to know that for those early investigators who speculated about the one- or two-fluid electrical theories we can now see that it did not make much 'sense to speak *separately* of the concepts of charge in terms of the electric fluid and the law of conservation of charge' (p. 366, my italics). Again the admitted lack of experimental evidence in favour of either theory, as this fact appeared in the eyes of the eighteenth- and nineteenth-century 'electricians', is of great importance to the philosopher of science when he comes to discuss the concept of 'mere mathematical hypothesis' (p. 370).

Sometimes the historians seem to go even further in their claim of the historical relativism of philosophical theory. Thus, Cohen, in the same paper, believes it to be provable that 'the philosophical point of view of such men as Stallo, Mach, Duhem, Poincaré, and others, is only mistakenly considered in relation to the subsequent development of physics. Their philosophy was much rather a reaction to the problems that had been raised by the science of their own century' (p. 371). I profess that I am puzzled by this remark. It may of course be true that Mach's views were stimulated by reflections on the nineteenth-century physical scene. But it hardly *follows from this* that Mach's ' philosophical discoveries ' (if I may call them thus) were *not* both true, and relevant for the physics of the twentieth century—even if *in fact* they should turn out to have been a muddle.

Professor Kuhn also, at one point, seems to be intent on giving the minimum of credit for the possibility that certain people at a critical stage might have been motivated by perfectly general and not by accidental historical influences. Among the three factors responsible for the conservation of energy principle he rightly singles out one of a ' philosophical ' kind which he calls ' philosophy of nature '. As he understands this, however, it means that specific stream in German philosophy at the beginning of the last century going under that name. He then argues (p. 338) that since the ' metaphysical counterpart ' of the dynamical conservation theorem must have been next to forgotten during the third decade of the nineteenth century, ' Naturphilosophie ' with its emphasis on the convertibility of natural forces must alone have been responsible for the discovery of the conservation principle. However, it should be said that one of its discoverers, J. R. Mayer, certainly disclaimed all association with this philosophy. But this does not stop Professor Kuhn. Though ' Mayer did not study Naturphilosophie . . ., he had close student friends who did ' (p. 339).—But why should Mayer not have been under the influence of quite *general* considerations, as he explicitly claimed that he had been, when again and again stressing, both in his writings and his letters, the central importance of the causal principle, expressed as ' cause = effect '? Once one gets a clear appreciation of what are and what are not genuine causes (i.e. forces) in nature, he wrote to Griesinger (30.11.42), then ' the drivel of the nature-philosophers is put in the pillory in all its miserable nakedness '. (Weyrauch, ed., *Kleinere Schriften*, 1893, p. 181). In other words Mayer could think for himself. It wasn't just (as Professor Carl Boyer remarks on this point) that Mayer could read, and that there were libraries! (cf. p. 389).

Of course, there is a reason for Boyer's criticism. For he has even less sympathy than Kuhn with the ' philosophical ' aspect. When Kuhn refers to this aspect as an attempt to give a ' reasonable justification ' (p. 386), Boyer understands this ' to mean some clear-cut mathematical or experimental justification ' (ibid.). No wonder he thinks that ' the maxim causa aequat effectum ' does not ' settle the problem '! He is very suspicious of all this sort of talk, just as he takes Kuhn to task for having declaimed (surely correctly?) that ' energy *is* conserved; nature behaves that way ' (p. 323). This, to Boyer, ' seems to imply a notion of energy as something actually *in* nature which we discover, rather than as a concept which we invent as one appropriate means of describing nature ' (p. 385). Shades of Mach! The same Mach whose ideas Professor Cohen seems to be wanting to tame by exposing their historical relativity with respect to nineteenth-century physics!

As I suggested in my remark on Father Clark's paper, we are not at all agreed on philosophical matters in science. And there is, as the later papers show, an even greater room for doubtful and tentative solutions, if we realise that historical interpretations play into epistemological solutions, and that at the same time, the former (e.g. of what Mayer did or did not mean or say or do) themselves live under the cloud of the traditional philosophical issues.

There are few books at the present time which will drive home so forcibly these lessons; a fact made particularly easy by the excellent feature of printing the replies to the various papers as well as the originals. Let us hope that there will be many successors.

GERD BUCHDAHL

Fads and Foibles in Modern Sociology and Related Sciences.
By Pitirim A. Sorokin.
Mayflower Publishing Co. and Vision Press, London, 1958, viii + 357. 50s.

PROFESSOR SOROKIN is a very angry old man who now stands disregarded alongside the growing edifice of American social science, in the ground-floor of which he was a master builder. He began writing as a sociologist in Russia before the First World War. Between the wars he was the author of the best known standard work on sociological theories and of a definitive treatise on social mobility which, though it has often been neglected, has yet to be supplanted. Since the Second World War he has increasingly devoted himself to the study of the principles and development of altruistic love and creativity (e.g. *The Ways and Power of Love*, 1954). In this his latest book, however, he turns aside from his more amiable pursuits to fire a violent cannonade into the office and laboratories of the above mentioned building. He offers us, in other words, an armed excursion into the philosophy of science.

The main artillery is directed on to American empiricism in sociology and psychology but other fads 'and foibles including amnesia (especially of Sorokin) and the discoverer's complex, obtuse jargon and sham scientific slang' enable him to define the target widely enough to include most members of the Harvard Department of Social Relations over the last generation and notably Talcott Parsons whose essays on social stratification for example are described as 'largely useless for empirical research' the problems being 'submerged in a sea of ponderous ruminations' (p. 324). English readers, especially of the *Times Literary Supplement*, will doubtless enjoy all this as a rumbustious knock-about turn from the other side of the Atlantic. In fact it is done with much greater economy and elegant wit by Ernest

Gellner in a review of the 1956 American edition of the book in the current number of *Inquiry*.

Sorokin lambastes the empiricists as quantophrenics and testomaniacs. Those who fear that the scientific skill of the psychometrician will facilitate the rise of the meritocracy will take comfort from Sorokin's view that 'if the current over-evaluation of tests is continued, one of its results will be an increasing misselection and maldistribution of individuals in various social strata and positions'. But the centre of the target consists of slavish imitative worship of that outmoded deity Newtonian macro-physics. (That is why Gellner describes modern American social science as a cargo cult.) The 'cult of numerology' and the 'bootlegging of mathematical formulae', indeed most of the testing, measuring, surveying and experimenting of contemporary sociology are nothing more than the vain repetitions of the heathen. Examples are embarrassingly frequent and familiar and Sorokin exposes them in all their inglorious splendour.

The task of demolition thus energetically completed, the author lays out his plan for re-construction—'the integralist conception of reality, knowledge and ways of cognition'. Here he is much less satisfactory. His greatest emphasis is laid on the importance of the rôle of intuition in science—'the creative intuitional flashes of the great geniuses that . . . reveal to them the essentials of a scientific discovery or of a creative masterpiece, are also of the same nature as the state of *samadhi* of a genuine yogin' (p. 290). But the reader will get a very much clearer and more comprehensive view of the methodological problems of induction, deduction, and theory construction from the works of Karl Popper which, though they constitute the most important treatment of the philosophy of the social sciences to appear in the present century, receive no mention from Sorokin. Popper's discussion of predictability is similarly much superior to Sorokin's, which appears to make prediction a matter of tense and fails to see its logical identity with explanation. Moreover, as Jean Floud has pointed out,[1] no amount of Sorokin's 'superconscious intuition' can ever dispense with the need to test generalisation against fact—a process which involves quantification, testing and experiment.

A. H. HALSEY

The Anatomy of Judgment. By M. L. Johnson Abercrombie. Hutchinson, London, 1960. Pp. 156. 25s.

THE subtitle to Mrs Abercrombie's book is 'an investigation into the processes of perception and reasoning'. This investigation was carried out on medical students and includes a description of a course that they received

[1] *British Journal of Educational Studies*, 1957, 6

which was designed to help them to make accurate observations, to draw reasonable influences from and to make critical evaluations of scientific data. During this course it was discovered, especially by the students, that each had his own schemata, or set of expectations which delimited what they saw as well as what they inferred. This discovery by the students was made during skilfully directed group discussions. The discussions were, technically, free discussions in that any member of the group could say what they liked. However, the skill shown by the investigator involved a great awareness of the techniques of group psychotherapy, and it is in this sense that the direction took place. A less aware teacher could have ruined the discussions.

Because the book looks as though it is an account of a new and fruitful teaching method, some readers of this *Journal* might miss the fact that it is an excellent account of the psychology of scientific method. I recommend the book to anyone who still thinks that scientists are completely inductive. The findings are excellent illustrations that the mind is not what Professor Popper has called an empty bucket, and that the basic process in science is the critical evaluation of our own conceptual framework.

From the practical point of view, I am sure that the course given to these students will turn out to be the most important part of their training.

R. F. J. Withers

ANNOUNCEMENT

British Society for the Philosophy of Science

Sixth Annual Conference: 22nd-24th September 1961

The Sixth Annual Conference of the British Society for the Philosophy of Science will be held at St Hugh's College, Oxford, from 22nd to 24th September 1961. The programme will be as follows:

'Explanations in Psychology', Dr M. Treisman, Dr J. O. Wisdom.
'Learning Machines', Dr F. George, Professor D. M. MacKay.
'Genetic Coding and Information Theory', Dr I. Leslie, Dr S. Brenner.
'Statements about the Universe', Mr R. Harré, Dr W. Davidson.

Details may be obtained from Dr M. B. Hesse, Whipple Museum, Free School Lane, Cambridge.

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UNIVERSALITY AND NECESSITY *

WILLIAM KNEALE

IN THIS paper I wish to consider some attempts which philosophers have made to define the notion of necessity by reference to universality. I think these attempts are mistaken, and I shall put forward some arguments in justification of my view; but my main purpose is not so much to argue a case against the suggested definitions as to bring them together and to point out some similarities and differences which have not been sufficiently noticed. I think that by so doing I may perhaps arouse doubts in the minds of some who have not felt doubts on the subject before. But apart from that I shall be glad to hear a discussion of the subject, and that is my main reason for choosing it.

I

The first reductionist attempt to which I wish to draw attention is that of Hume. In his analysis of the notion of causation he remarks that this relation is supposed to involve necessary connection and asks whence the idea of such connection can be derived. The question seems puzzling to him because he has committed himself to the principle that every simple idea is derived from a simple impression and yet finds himself driven to say that no necessary connection can ever be discovered by perception between events we call causes and events we call effects. After a lot of beating about the neighbouring fields he comes in the end to the conclusion that the necessity of which we speak in this connection is merely a projection upon the world of a feeling of inevitability which we have when we pass in thought from a cause to its effect, and that this feeling of inevitability is the result of an association established by constant conjunction of instances of the first with instances of the second.

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* Read to the Annual Conference of the British Society for the Philosophy of Science held at Bristol, September 1960.

Now this is not strictly speaking a definition of causal necessity in terms of universality, but it is certainly an attempt to explain away the necessity of which many philosophers have spoken in this connection. Hume does not for a moment deny that in ordinary life and in science we talk of necessary connection in nature, but he tries to explain how this can be so without admitting that there are in truth any necessary connections in nature, and he thinks that the key to an understanding of the situation is recognition of the importance of the constant conjunction of the events we call cause and effect. As though to mark the crucial importance of this notion, he prints the words ' constant conjunction ' in small capitals at their first appearance.

Probably no modern philosopher would wish to defend Hume's doctrine of causation just as it stands. It is now obvious that he made a mistake in talking of antecedent causation as the only relation between events by reliance on which we can make inferences from the observed to the unobserved. It is obvious also that what he says about causation does not suffice to distinguish a cause from a sure sign of something to come. And his psychological account of the origin of our talk of causal necessity is not likely to find much favour with anyone now. But there are a great many modern philosophers who think that Hume was right in principle; and if asked to say just what it was he contributed here to philosophy, they reply that he freed the notion of natural law from a confusion with logical necessity in which it had been involved by previous philosophers. These modern followers of Hume do not, of course, wish to deny that we often talk of an event's being made necessary by another, but they think they can explain this by an adaptation of Hume's argument. Instead of saying that talk of necessity is projection of our feelings on the world, they say that it arises in connection with the use of natural laws as premisses for inference and is quite correct so long as it is understood in that connection. When I find something which is A, I may properly say that it must be B, if I am speaking to people who are already convinced that every A thing is B. For in this context the word ' must ' serves only to indicate that my assertion is an inference from the premisses ' Every A thing is B, and this is an A thing.' But, they say, philosophers have sometimes mistakenly supposed that the word ' must ' indicates a necessity in the law itself. Frege seems to have held this view of natural necessity when he excused himself in his *Begriffsschrift* for not dealing with modal notions. There is a very clear presentation of this view in Professor Popper's contribution to a symposium on the question, ' What can Logic do for

Philosophy?', in the *Aristotelian Society Proceedings, Supplementary Volume for* 1948. I do not think it is unfair to describe it as a modern version of Hume's theory and to say that it is an attempt to explain physical or natural necessity by reference to universality.

Historically the doctrine that there is no necessity in nature itself has often been associated with a conventionalist account of such necessities as men claim to know *a priori*, but I do not think it is quite correct to father the conventionalist thesis on Hume. When he spoke of relations between ideas and contrasted them with matters of fact, he may sometimes have meant relations of inclusion such as would be revealed by definitions; but I think that he sometimes had in mind relations of positive opposition, for example between colours, and that his philosophy contained in fact a remnant of Locke's doctrine of *a priori* knowledge as knowledge of relations between ideas. So far as I know, the first clear statement of the conventionalist thesis in conjunction with a denial of necessity in nature is to be found in Berkeley's *Philosophical Commentaries*, §§ 732-5, where he writes:

> The reason why we can demonstrate so well about signs is that they are perfectly arbitrary and in our power,—made at pleasure. The obscure ambiguous term *relation*, which is said to be the largest field of knowledge, confounds us, deceives us. Let any man show me a demonstration, not verbal, that does not depend either on some false principle or at best on some principle of nature which is the effect of God's will and we know not how soon it may be changed. *Qu:* What becomes of the *aeternae veritates*? *Ans:* They vanish.

In recent years the thesis has been popularised by Wittgenstein. Whether or not he intended to preach a conventionalist theory of *a priori* knowledge when he published his *Tractatus*, I do not know: there are some indications that he did not. But that was the interpretation which his disciples put on his work, and he certainly held the doctrine in his later years. Because he said that all necessity was logical necessity, what we claim to know *a priori* is often now said to be all logical, even though some of it is not obviously connected with the subject studied by Aristotle and Frege. I think this usage has misled a lot of philosophers, but there is no need to discuss it here.

2

So far what I have said is common knowledge. But I come now to views that are not so well known—those of Bolzano and Tarski about logical necessity. In his *Wissenschaftslehre* of 1837 Bolzano produced an account of logic as the science of sciences, which is very different from the psychologising empiricism of Hume. Because he was a Roman Catholic priest and a professor of the philosophy of religion in Prague until deposed for expressing liberal opinions on politics, it has sometimes been supposed that he derived his characteristic views from medieval sources. He may no doubt have been influenced by reading of medieval authors, but he does not often quote them in his *Wissenschaftslehre*. In fact I have found there only three references to medieval philosophers, and those not very important. On the other hand there are many references to 'the great Leibniz', and it seems to me reasonable to think of Bolzano as working in the Leibnizian tradition, though, at the time he wrote, most of Leibniz's important work on logic was still unpublished. In various places Leibniz had pointed out that conventionalism was an unsatisfactory theory of logical necessity, since even conventions such as definitions must be shown to be consistent, and in his *Dialogue on the Connexion between Words and Things* he had ridiculed the suggestion that truth could be said to belong to sentences considered as patterns of ink. In his view, if we talk of the truth of sentences (i.e. *propositiones* in his Latin), we must consider not only actual sentences but also possible sentences, since there may well be undiscovered truths, and this introduction of talk about possible sentences shows that we are not really concerned with the speech habits of human beings but with something deeper which is presupposed in any attempt to regulate speech habits. Bolzano talks in this context of *Sätze an sich* (or propositions in the modern sense of that word). The phrase is difficult, if not impossible, to translate into English, because we have no word with precisely the same range of uses as the German *Satz*. Like our 'statement' it can mean either a form of words or what a form of words expresses, but unlike 'statement' it need not be confined to the context of assertion. Sometimes it has the sense of our 'clause' as in the grammatical term *Nebensatz*. Sometimes, on the other hand, it has the sense of 'thesis' or 'principle' as in the phrase *der zweite Satz der Wärmelehre* for 'the second law of thermodynamics'. I mention all this for two reasons, first in order to show that Bolzano did not belong to the Berkeley-Hume-Mill tradition but rather to what one might call the tradition of logical realism of which Leibniz and Frege are the greatest representatives, and secondly in order to explain his terminology.

According to Bolzano a proposition (i.e. a *Satz an sich*) is analytic with respect to a certain constituent if the class of propositions that we can obtain by substitution for that constituent (including here under

' substitution ' replacement of the original constituent) consists entirely of true propositions or entirely of false propositions. Bolzano himself admits that the notion of analytic proposition which he has defined in this way is much wider than that of Kant; and he says that, if the part of an analytic proposition which is considered invariant (i.e. not open to substitution) contains only logical notions, it may perhaps be useful to describe the whole proposition as logically analytic, or analytic in the stricter sense (*Wissenschaftslehre*, §§ 147-148). Apparently he thinks that this is something like the sense intended by Kant; but he does not attach much importance to it, because he does not think it possible to draw a clear line between logical and non-logical notions.

There are a number of curious features in this passage of Bolzano's work. The first is a purely verbal point. Bolzano has so defined the word ' analytic ' that it can be applied to false propositions. This is contrary to Kant's usage, but it is easy to see why Bolzano thought it reasonable. He wanted to use the words ' analytic ' and ' synthetic ' as exhaustive of the realm of propositions, and he noticed that according to Kantian usage self-contradictory propositions would be neither analytic nor synthetic. The practice of later philosophers has followed Kant's lead rather than Bolzano's and it seems that we must either reconcile ourselves to saying that ' analytic ' and ' synthetic ' are not contradictory opposites or try to secure the exhaustiveness of the division by applying it only to truths. Secondly, Bolzano's account of analytic propositions seems to be based on the very naive assumption that a proposition must contain distinguishable constituents corresponding to all the distinguishable constituents of a sentence that expresses it, and *vice versa*. I shall say no more about this. Thirdly, Bolzano has given a definition of ' analytic ' such that a proposition can be analytic by virtue of natural laws or even by virtue of mere accidents. Consider for example the sentence ' Kant was not an eighteenth-century philosopher who died on the anniversary of his birth.' If it so happens that no eighteenth-century philosopher died on the anniversary of his birth, all the sentences that can be made from this by substitution of other names for ' Kant ' express true propositions, and so the proposition expressed by the original sentence is analytic with respect to the constituent Kant according to Bolzano's definition.

The most interesting and valuable element in Bolzano's discussion of the distinction between analytic and synthetic propositions is his recognition of the fact that there are certain characters which belong to propositions in virtue of their structure. According to his way of talking a proposition may be universally valid (*allgemeingültig*) and so analytic with regard to substitution for certain constituents though not with respect to substitution for certain others. This is intelligible, but rather confusing, and it seems preferable to say (as later logicians have done) that universal validity (or validity for short) belongs primarily to certain propositional patterns and only secondarily to the propositions which exemplify them. Similar paraphrases can be applied also to what Bolzano says of compatibility and derivability.

According to Bolzano's own way of describing the matter, the propositions M, N, O, . . . follow or are derivable (*ableitbar*) from the propositions A, B, C, D, . . . with regard to the constituents i, j, . . . if any set of ideas which yield a set of true propositions when substituted for i, j, . . . in A, B, C, D . . . do the same when substituted for i, j . . . in M, N, O, . . . (*Wissenschaftslehre*, § 154). In other words this means that a proposition called a conclusion is entailed by a set of propositions called premisses if the argument constituted by the association of the premisses and the conclusion exemplifies a pattern of argument for which every exemplification with true premisses also has a true conclusion. For a reason which I shall notice later, it is not now considered correct to identify following-from with being-derivable-from, as Bolzano does, but without going into details of modern discoveries we can see that there is something wrong in the use of the word ' derivability ' for the relation defined by Bolzano. For a proposition cannot properly be said to be derivable from a set of premisses unless it is possible to establish that if the premisses are true the proposition is also true without first establishing whether or not the premisses and the proposition are true. But the relation defined by Bolzano might hold when this condition was not fulfilled. Just as according to his definitions a proposition can be analytically true by accident, so too one proposition may follow from another by accident, that is to say in such a way that the truth of the universal proposition about the results of substitution can be known only by an examination of the individual results. This can be seen most easily from consideration of the limiting case where the proposition called a consequence has no constituents in common with any of the premisses. For then, according to Bolzano's definition, its being a consequence of the premisses depends solely on its being true. He does not make this point explicitly in his section on the relation of being a consequence, but in his previous section on compatibility he says that a false proposition which contains none of the constituents for which substitution may be made in a certain set of propositions is incompatible with those propositions (*Wissenschaftslehre*, § 154), and according to his own account of the connection between the two sections this is just another way of saying what I have just said about being a consequence. Perhaps Bolzano overlooks it because he wants to think of the relation of being a consequence as holding always in virtue of a general rule. For he says in a note at the end of this section that when Aristotle uses the phrase *συμβαίνει ἐξ ἀνάγκης* (' it follows of necessity ') to describe the relation of the conclusion to the premisses in a valid syllogism, even though premisses and conclusion may be alike false, he must surely mean that every argument of the form exemplified leads to a true conclusion if only the premisses are true.

All this might be dismissed as of no more than antiquarian interest were it not for the fact that Bolzano's theory has been restated independently in our time by Tarski. In his article ' On the Concept of Logical Consequence ' (first published in Polish in 1936 and republished in English in *Logic, Semantics, Metamathematics*, p. 409) he rejected the Wittgensteinian account of logical necessity as unclear and said that Gödel's discoveries had shown the need for a new account of the relation of consequence:

> In order to obtain the proper concept of consequence, which is close in essentials to the common concept, we must resort to quite different methods and apply quite different conceptual apparatus in defining it.

The method which he then proposed was in essentials that of Bolzano, though at the time of his first publication on the subject he knew nothing of this part of Bolzano's work.

Tarski himself has summarised his account of the consequence relation as follows:

> Let *L* be any class of sentences. We replace all extra-logical constants which occur in the sentences belonging to *L* by corresponding variables, like constants being replaced by like variables, and unlike by unlike. In this way we obtain a class *L'* of sentential functions. An arbitrary sequence of objects which satisfies every sentential function of the class *L'* will be called a *model* or *realization of the class L of sentences*. . . . If in particular the class *L* consists of a single sentence *X*, we shall also call the model of the class *L the model of the sentence* X. In terms of these concepts we can define the concept of logical consequence as follows:
> *The sentence X follows logically from the sentences of the class K if, and only if, every model of the class K is also a model of the sentence X.*

Later he adds:

> We can agree to call a class of sentences *contradictory* if it possesses no model. Analogously a class of sentences can be called *analytical* if every sequence of objects is a model of it. Both of these concepts can be related not only to classes of sentences but also to single sentences.

Tarski himself insists that his essay is an attempt to work out clearly the implications of the old doctrine that the relation of logical consequence holds between statements in virtue of their forms. But his development of the doctrine has some curious features. I wish to draw attention to one only, and for this purpose I shall quote again from his own work. At the end he writes:

> Underlying our whole construction is the division of the terms of the language discussed into logical and extra-logical. This division is certainly not arbitrary. . . . If, for example, we were to include among the extra-logical signs the implication sign, or the universal quantifier, then our definition of the concept of consequence would lead to results which obviously contradict ordinary usage. On the other hand, no objective grounds are known to me which permit us to draw a sharp boundary between the two groups of terms. It seems to be possible to include among logical terms some which are usually regarded by logicians as extra-logical without running into consequences which stand in sharp contrast to ordinary usage. In the extreme case we could regard all terms of the language as logical. The concept of *formal* consequence would then coincide with that of *material* consequence.

By ' material consequence ' he evidently means here the converse of what logicians call material implication. We may add that in this extreme case the concept of the analytic would coincide with that of the true.

In effect Tarski expresses the same doubts as Bolzano about the possibility of drawing a clear line between the logical and the extra-logical; and having made this point he rightly goes on to show that by placing the boundary between formal and material in different places we can pass from the ordinary notion of logical consequence to much weaker notions. For just as many philosophers who profess to follow Hume have said that the necessity with which an effect follows its cause is no more than the inclusion of this particular sequence in a constant natural association, so he says that the necessity with which a consequence follows in the logical sense of 'follows ' from premisses is no more than the inclusion of this particular sequence in a universal fact about the satisfaction of certain sentential functions. If, following his own suggestion, we enlarge the range of sentential functions under consideration by including among the constant factors of our sentences not only the traditional logical constants but all unrestricted general terms such as 'man' 'iron', 'fire', etc., we find that his definition of consequence covers not only logical consequence but also all consequences in virtue of natural laws. In short, his account of necessity amounts to a generalisation of the constancy theory of natural necessity. But the result can give no pleasure to followers of Hume, since it involves rejection of their view that there is a fundamental difference of kind between logical and natural necessity.

In defence of Tarski's definition it may perhaps be argued that for a sentential function such as 'If p then not-not-p' we can know *a priori* that it is satisfied by every sequence of objects, whereas for a sentential function such as 'If x has taken arsenic x will die' we can only conjecture universal satisfaction on empirical grounds. This is true, but there is nothing in Tarski's account of the matter to explain the difference, and he does not allude to it himself. Nor is it appropriate that a distinction of kinds of consequence should be made to depend on a distinction between cases in which we can and cases in which we cannot gain knowledge *a priori*. On the contrary, the epistemological distinction should be explained by an account of the difference of the cases; and it is just this which is lacking so far.

3

For my own part I do not agree with the project of trying to define necessity by reference to universality, and I have argued against even the more popular part of the project, namely that of accounting for natural necessity by a modernised version of Hume's analysis. In my *Probability and Induction* of 1949 I pointed out that when we enunciate a natural law, or what we suppose to be such, in the form 'Every F thing is G', we do not think of it as a merely *de facto* generalisation of the kind logicians express by writing '$(x)\ [Fx \supset Gx.]$'. Although we may not use any modal word such as 'must' or 'necessarily', we assume that our pronouncement commits us not only to asserting that everything which actually has been or will be F has been or will be G but also to asserting that if anything which is not as a matter of fact F were F it would also be G. This, I thought, was sufficient to show the inadequacy of a Humean account of natural law. The same point has been made by a number of other philosophers, but the conclusion which I have drawn has not been accepted by all, or indeed by many. Although it seems to be generally agreed that subjunctive conditional statements can be inferred from statements of natural law, and that statements of natural law must for this reason be distinguishable from statements of accidental universality such as we find in works of history and geography, for example ' All mountains in the United Kingdom are of less than 5,000 feet in height ', it is sometimes thought that this feature of statements of law can be explained without supposing that they involve any more than Hume allowed.

Already in 1948 Professor Popper argued for this view in the symposium ' What can Logic do for Philosophy? ' and he defended it again in 1949 in ' A Note on Natural Laws and so-called " Contrary-to-fact " Conditionals ' which he contributed to *Mind.* His thesis was that the difficulties of persons like myself arose from failure to notice the difference between terms which can be defined extensionally and those which cannot be so defined. It is true, he said, that ' All my friends speak French ' does not entail ' If Confucius were one of my friends he would speak French ', but that is because anyone who utters the first statement is thinking of the class of his friends as closed, whereas anyone who utters the second statement is thinking of the class of his friends as open. In other words, the expression ' my friends ' is not used in quite the same way in the two statements. When, however, it is maintained that sentences which purport to state natural laws are equivalent to universal material implications, it is to be understood that the terms involved are not mere substitutes for lists of proper names but unrestricted general descriptions. And once this is conceded, there should in his opinion be no difficulty about the derivation of contrary-to-fact conditionals.

Against this I argued in a paper contributed to *Analysis* in 1950 (later reprinted in Miss Margaret Macdonald's collection *Philosophy and Analysis*) that Popper's theory would not do what he wanted of it because it still allowed for no distinction between laws and merely accidental generalities. Philosophers who treat suggestions of law as universal material implications say in effect that there is no sense in talking of historical accidents on the cosmic scale. According to their account of the matter, when we consider the hypothesis of a connection between any two characters expressed by unrestricted general descriptions, there are only two possibilities with which we have to reckon. Either it is a law of nature that every A thing is B, or there has been or will be somewhere at some time an A thing that was not or is not B. In short, they are committed to the view that every natural possibility (i.e. every state of affairs not excluded by a law of nature) must be realised somewhere at some time. But this is certainly not what we ordinarily think, and any philosophical theory which leads to this conclusion should be regarded with great suspicion.

In an appendix to his *Logic of Scientific Discovery* of 1959, Popper concedes that something more must be done to distinguish laws from accidental generalities and produces the following definition of natural necessity:

> A statement may be said to be naturally or physically necessary if, and only if, it is deducible from a statement function which is satisfied in all worlds that differ from our world, if at all, only with respect to initial conditions (p. 433).

As he goes on to remark, this proposed definition makes all laws of nature, together with all their logical consequences, naturally or physically necessary, but excludes from that status generalisations which hold merely because of the *de facto* arrangements of things in the world we know. Personally I find it acceptable. For to say that a statement function is satisfied in all worlds that differ from the actual world, if at all, only with respect to initial conditions is to say in effect that it holds for all *possible* worlds that contain instances of the same attributes and relations as are exemplified in the actual world and of these only; and what holds for all possible worlds is obviously necessary. I agree, of course, with Popper that we cannot know for certain whether a generalisation which we put forward is in truth a law of nature with natural necessity as he has defined it; but to say this is only to admit that we cannot establish laws of nature *a priori*. Obviously experience, which is always of the actual world, cannot guarantee a generalisation which is supposed to hold for worlds other than the actual world, and those who speak, as I have done, of natural necessity do not wish to say that it can. The important thing in Popper's new definition, and what makes it acceptable to me, is just that it connects the notion of natural law with that of validity for states of affairs other than the actual. Unfortunately, however, Popper himself seems to be in some confusion about the effect of his concession. Perhaps I have misunderstood him. If so, I hope to be enlightened. But some passages which I am going to quote seem to me very puzzling indeed.

Just before his new definition of natural necessity Popper writes:

> As Tarski has shown, it is possible to explain *logical necessity* in terms of universality: a statement may be said to be logically necessary if and only if it is deducible (for example by particularisation) from a 'universally valid' statement function, that is to say, from a statement function that is *satisfied by every model* (this means true in all possible worlds). I think we may explain by the same

> method what we mean by *natural necessity*; for we may adopt the following definition (*L.S.D.* p. 432).

But a few pages later he says:

> I regard, unlike Kneale, ' necessary ' as a mere word, as a label for distinguishing the universality of laws from ' accidental universality '. Of course any other label would do as well, for there is not much connection here with logical necessity. I largely agree with the spirit of Wittgenstein's paraphrase of Hume: ' A necessity for one thing to happen because another has happened does not exist. There is only logical necessity ' (*L.S.D.* p. 438).

As they stand, these two passages are inconsistent. For the first recommends a new account of natural necessity by saying that it is framed after the pattern of Tarski's account of logical necessity, whereas the second says there is no important connection between natural necessity and logical necessity. And the first implies acceptance of Tarski's account of logical necessity, whereas the second implies acceptance of Wittgenstein's account of logical necessity, which Tarski himself, as we have seen, found unsatisfactory. I do not wish, however, to dwell on the second passage, and I quote it here only in order to indicate that there seems to be some uncertainty in Popper's own mind about his commitments. It is the first passage which interests me, and it is this I wish to examine.

At the beginning Popper says that Tarski has shown that it is possible to explain logical necessity in terms of universality. I think this is at any rate a correct report of Tarski's intention. But, as I have already remarked, neither Bolzano nor Tarski ever produced a delimitation of the logical realm which satisfied him, and there is something unsatisfactory in a theory of logical necessity which does not even explain why logic is an *a priori* science. It is obvious, of course, that truths of logic are either themselves universal in the way explained by Bolzano and Tarski or cases falling under generalisations of the kind those authors discuss. That has been taken for granted by all the great logicians, Aristotle, Chrysippus, Leibniz, Boole, Frege, and Russell. But it is not so obvious that there is nothing more to be said about logical necessity.

Towards the end of the passage I have quoted, Popper tries to make the theory more acceptable by adding in brackets ' this means true in all possible worlds '. This phrase is supposed to explain the immediately preceding phrase ' satisfied by every model ', but according to Tarski's own explanation a model is an arbitrary sequence of objects which satisfies a statement function or class of statement functions, and an analytic statement is one for which every sequence of objects is a model, i.e. one satisfied by every sequence of objects, or as we may say in another terminology, one which turns out true for every interpretation of its extra-logical signs. There is nothing here about possible worlds, and it seems clear to me that, so far as Tarski is concerned, the truth of a formula under all interpretations is just a fact about the actual world, though this actual world may be taken to include not only physical objects but an infinity of sets belonging to various levels. Indeed, if Tarski's programme is what Popper says at the beginning of the paragraph, namely to explain logical necessity in terms of universality, he has no right to speak of possible worlds at all. It was no doubt correct for Leibniz to say that necessary truths are true in all possible worlds, and even to offer this as a definition of necessity; but he did not think that he was explaining necessity in terms of universality and so showing the superfluity of modal expressions. For him the definition was merely an explanation of one modal notion in terms of another which some people find easier to grasp. If Tarski were willing to work with the notion of possibility as Popper supposes, he could define the notion of consequence much more easily than he has done, by saying simply that a statement is a consequence of a class of premisses if the conjunction of the premisses with the negation of the statement does not represent any possible state of affairs. But I feel sure that he would think this a less satisfactory definition than that he has given, precisely because it contains a modal word.

Immediately after the sentence about possible worlds Popper goes on to say 'I think we may explain by the same method what we mean by natural necessity', and the explanation that he gives is indeed an explanation of the Leibnizian type. For it involves generalisation over all worlds that differ from our world, if at all, only in initial conditions, and these must clearly be naturally possible worlds with instances of the same attributes and relations as we find exemplified in our actual world. I hold therefore that Popper is mistaken if he thinks, as apparently he does, that he has succeeded in improving on the Humean account of natural laws while remaining true to the principle that necessity can be explained without remainder in terms of universality. In fact he has only defined one modal notion in terms of another; but he has hidden this from himself for the moment by talking of all worlds which differ from our world, if at all, only in initial conditions.

In my opinion, it is a mistake to try to explain away the modal notions. Instead of trying to reduce necessity to universality we should, I think, take the notion of necessitation as fundamental and say that the logical constants, about whose recognition Bolzano and Tarski were both puzzled, are just those signs which can be defined without remainder by formulation of principles of necessitation. Popper himself has taken this line in a number of papers written about 1947–8 (in particular two papers ' On the Theory of Deduction ' which he contributed to the *Proceedings of the Royal Netherlands Academy of Sciences* in 1948), and I have tried to follow him in a paper called ' The Province of Logic ' which I contributed to *Contemporary British Philosophy, Third Series.* What I suggest is simply that we should regard formal logic as the pure theory of necessitation, that is as the study of what can be said about necessitation in general without regard to those special principles of necessitation which hold for various subject matters. Although he has come very near to saying this, Popper has in the end drawn back and professed himself a follower of Hume and Wittgenstein, that is, an upholder of the anti-scientific doctrine of conventionalism which was first promulgated in modern times by Berkeley. His reason for taking the line which he does is apparently that he cannot understand what a non-formal principle of necessitation would be and suspects that admission of any such would commit him to a terrible evil called essentialism. To this I reply that a non-formal principle of necessitation would be exactly what he has allowed a law of nature to be, namely a generalisation which holds for all possible worlds of some kind, and that it is highly paradoxical to suggest, as he does in a passage which I quoted, that there is none but an arbitrary linguistic connection between natural and logical necessity.

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ON THE RELATIONSHIP BETWEEN METHODOLOGY IN SCIENTIFIC RESEARCH AND THE CONTENT OF SCIENTIFIC KNOWLEDGE *

DAVID BOHM

SCIENCE is characterised in an essential way by what is commonly called the *scientific method.* The main content of this method is a set of general rules, criteria, and directives, partially implicit and partially explicit, as to how to set about doing scientific research, to evaluate the results obtained in doing so, and to modify one's subsequent work on the basis of such evaluations. For example, one should do experiments, set up hypotheses to explain their results, test these hypotheses by experiment, discard those that are not confirmed, set up new hypotheses to explain the new experiments, etc.

Scientists generally apply the scientific method, more or less intuitively, and can usually tell in practice whether they are following it properly or not. Nevertheless, the question of proper method becomes ambiguous under certain circumstances. This happens especially in the development of new sciences, and in fields that are on the border line between science and something else (e.g., psychology, sociology, etc.). Even in traditionally scientific fields, however, the question of method sometimes becomes unclear. For example, in the growth of relativity and quantum theory a great deal of attention had to be focused on the meaning of measurements and on the proper rôle of theories (to such an extent, for example, that ether theories were discarded partly for the reason that they were criticised as 'metaphysical' because the velocity of the ether was not capable of being measured).

The reason why the intuitive application of the scientific method becomes ambiguous in borderline regions is basically that all the terms which appear in its rules, criteria, and directives depend for their clear meaning on what may be called a general framework of concepts, ideas, procedure, etc., which function together as a coherent whole. Against a background in which this general framework applies, one can consider specific problems which do not call the framework into question. Within this limited domain, it is clear how one is to go about finding the facts, setting up hypotheses, testing them, etc. But as we approach the edge of such a domain, these questions become less clear.

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* Read to the Annual Conference of the British Society for the Philosophy of Science at Bristol University in September 1960.

In order to bring out this point in more detail, let us consider briefly Professor Popper's well known thesis, that whereas a hypothesis claiming to have universal validity cannot be verified by any number of confirmations, it can be refuted by one falsification. In the domain where the general framework of a science is clear and well-formed, it is easy to see what constitutes a falsification. For example, it is obvious that the hypothesis that an object in a vacuum will fall at a constant velocity can be tested by an experiment, aimed at either falsifying or confirming it. This is because a very large number of things can already be accepted. Thus, one begins with the knowledge that there is a well defined frame of space and time, that an object occupies a definite position at a definite time, that its position can be measured in certain ways, etc. Within this framework, it is possible, so to speak, to ask of Nature a question which has a definite 'yes' or 'no' answer; for example, 'Does or does not the object occupy such and such a series of positions at such and such a series of times?'

However, as we approach the boundary of the domain where a given framework applies, then it is no longer clear what is the right question to ask. For example, in pre-relativistic theory, it was always valid to ask 'Did event A come before event B or after?' Relativity theory showed, however, that if events are outside each other's light cones, there is no unique answer to this question. The answer depends, in fact, on the speed of the frame in which space and time are measured. This is because space and time are different in properties from what was assumed in the Newtonian conception. Similarly, quantum theory showed that in the micro-domain, one can no longer answer 'yes' or 'no' to the question of whether there is an electron at a given point or not. This is because an electron is not just a particle, but has wave-like properties also. Examples of this kind can be found in a wide variety of fields.

It is clear then that underlying the problem of falsification and confirmation, there is something very much more fundamental; namely, that of framing questions that have a definite 'yes' or 'no' answer in a given situation. It is common in everyday experience that most questions cannot be answered with a clear 'yes' or 'no'. It is sometimes overlooked, however, that the same is generally true in science, and that a great deal of work is needed to arrive at a question that does have such an answer. In fact, it is frequently realised that half the battle is over when we know what are the right questions to ask.

What must be emphasised here is that the *form* in which a question is put constitutes an implicit hypothesis about the object of the question; namely, that the object is such that this question has a clear ' yes ' or ' no ' answer. The classical example of such a question is ' Have you stopped beating your wife? ' Evidently, this question implies that you are the sort of person who beats his wife. Similarly, the question ' Is the electron at a given point or not? ' implies that the electron is the sort of thing that can be at a given point. Most scientific questions are in some way similar, in the sense that whether the answer is ' yes ' or ' no ' is much less important than whether they do, in fact, have definite answers of this kind.

It is clear that an essential feature of the scientific method is that it seeks hypotheses leading to questions that can be answered definitely ' yes ' or ' no ' by experiments or observations. But since we recognise that a science may pass through phases (especially while it is developing rapidly) in which the proper questions having this character are not even known, we are led into a whole new series of problems relating to the rôle of concepts, theories, and hypotheses in scientific research. Among these, one of the most important is that of what can be meant by the term ' fact '. Now, as is well known, a fact is a contingency, in the sense that *a priori* it could either be so or not so. As a result, some empirical criteria are needed to decide in each case whether it is so or not. If, however, we are working in a domain in which the usual questions that have been asked of Nature do not have a clear ' yes ' or ' no ' answer, then our customary criteria of factuality become ambiguous. And where these criteria are ambiguous, we are not really dealing with facts, even though we may at first sight think that we are.

It is not at all uncommon for a scientist to be mistaken as to whether something which he takes to be a fact can really even be one. For example, a physicist not aware of the theory of relativity could say ' A possible fact is that events A and B are simultaneous for all observers '. Knowing the theory of relativity he would only say ' A possible fact is that they are simultaneous for an observer moving at a given velocity '. In terms of the quantum theory he could go further to say ' There is an irreducible degree of ambiguity in the relative time of two events (implied by the uncertainty principle), so that it may not even be a possible fact for two events to be simultaneous for a single observer.'

Thus, guided by different conceptions, one is led to seek different kinds of facts, some of which may be possible in a given field, and some not.

It follows from the above that facts are not like things that exist in nature independently of man, so that they can, so to speak, be gathered as if they were stones or flowers. Rather, as the derivation of the word indicates, they are *made* (or manufactured). In this regard, nature may be compared to the raw material; while the fact is a finished product, having a form determined in part by man. Like a pair of shoes, for example, the fact reflects both the material (i.e. nature) and the process by which it was made. However, even the form is not wholly at man's disposal, since not every material will sustain every form (e.g. shoes cannot be made out of water). Similarly, the content of each field of scientific research calls for facts having appropriate forms. As we enter new fields, we must generally give new forms to the facts, forms that will be appropriate to the character of nature in the new domain.

It is in the form of the facts that the whole body of the existing framework of concepts and laws plays a crucial rôle. For, as we have already seen in a number of examples, each such framework suggests certain questions which should have a 'yes' or 'no' answer. In the domain where a given framework is valid, all facts can ultimately, in principle, be expressed by giving such an answer to an appropriate set of questions. In this regard, even numerical facts have such a character. Thus every real quantitative measurement (which is necessarily of finite accuracy), answers the question 'Is a given parameter between certain prescribed limits or not?'

But, of course, it is not enough merely to list the facts. It is also necessary in the scientific method that the fact should be relevant. It is here that laws play a fundamental part. For, in scientific research the relevance of facts to each other generally consists in their being related in some coherent system of law. Indeed, it is a basic assumption underlying the scientific method that any set of facts can, in principle, ultimately find their places in a suitable system of law. Of course, it is not expected that this will ever really be accomplished in such a way as to include the totality of all possible facts. Therefore, this principle, like that of seeking questions having a definite 'yes' or 'no' answer, plays the rôle of a directive, showing in what way we should aim to develop our ideas.

At any given moment then, we are faced, not with one coherent system of facts, but rather with a large number of partial systems, each having some intermediate degree of coherence. (There are different fields of study, different domains and levels in each field, etc.). Within each partial system, certain kinds of facts are possible. For example, in the context of everyday experience, there is an enormous variety of facts (e.g. a bell rings, the sun shines, there is a speck of dust here, a cloud there, etc.). It is recognised that most of these facts are not relevant to each other, or to the expression of the laws of physics, chemistry, biology, etc. (except perhaps in an extremely remote way). In order to obtain facts that are relevant in a given field, we need a set of concepts and a corresponding set of laws, which enables us to express the facts *as being in the field in question.* Thus, to study classical dynamics, it is no use to consider the colour or texture of the objects (which are facts in the context of everyday experience). Rather the facts and laws must be expressed in terms of co-ordinates and velocities of various bodies. On the other hand, in quantum mechanics the facts and laws are expressed in a very different way, namely, in terms of energy levels, cross-sections, probabilities, wavelengths, parities, etc. Indeed, the whole methodology of modern physics has been developed largely in the effort to 'manufacture' facts of this general kind.

The laws of a particular science not only play their parts in a general scheme with the aid of which facts can be organised into a single body of knowledge; they also help to define new forms of facts that are relevant for the science in question. For each law implies a whole set (generally inexhaustible) of relationships in the totality of facts. *Each such relationship is itself a possible fact*, in the sense that whether it is present or not is a 'yes' or 'no' question, subject to experimental or observational test. Moreover, if a large number of different kinds of relationships is consistently found to be as implied by a certain law, then the applicability of that law is likewise taken to be a fact, at least for some domain, and to a suitable degree of approximation. Even if the law should be refuted in some other domain, or in experiments carried out to a higher accuracy, the fact of its validity in the original domain and degree of approximation is not thereby altered (e.g. as the physicist accepts as a fact the behaviour of large-scale apparatus approximately according to the laws of classical mechanics, although he uses this apparatus in studying the laws of quantum mechanics, with the aid of which he will refute the notion that the classical theory is exactly and universally valid.)

It is evident that the part played in scientific method by already known laws will also eventually be played by laws which are not yet known, but which will come to be known in the future. But a hypothesis is just a presupposition concerning such laws, a presupposition that when it is first made has yet to be confirmed or refuted. Since the scientific method implies that we should proceed *as if* the hypothesis were true, unless and until it is actually refuted, it follows that even a hypothesis can help organise existing facts into a coherent whole and determine new forms in which relevant facts can further be developed. For, if the hypothesis is correct, it leads us to ask appropriate questions of Nature. Consider, for example, the hypothesis of non-conservation of parity, which opened up for consideration a whole new set of possible facts relating to the properties of systems under space-time reflections, properties that would not have had any meaning in the framework of previous hypotheses involving conservation of parity. Even when the hypothesis is not correct, but just on the right general lines, it may, as has frequently happened in the history of science, suggest problems that stimulate fruitful research and eventually lead to better theories. On the other hand, there is, of course, the possibility also illustrated by many examples in the history of science, that wrong hypotheses may impede research, by drawing attention away from questions that are relevant. Thus, the organisation of facts and the determination of their forms with the aid of hypotheses has both great advantages and great dangers.

The form of facts is determined, not only by concepts, laws, and hypotheses, but also by certain still more general and pervasive assumptions, which are usually implicit in what I may call habits of thought that have become associated with scientific thinking. Two examples will be considered here; first, the habit of thinking in terms of prediction, and second, the habit of specialisation.

The tendency to focus on the predictive aspects of science became important with the advent of Newtonian mechanics and its application to ballistics, the movement of planets, etc. In this branch of science, facts generally took the form, as we have already pointed out, of positions and momenta of bodies at one or more instants of time, while hypotheses were made on the general laws of motion from which one could deduce positions and momenta at other times. The test of hypotheses came to be the matching of future observations with predictions. Gradually there arose the notion that the ideal toward which physics should aim is complete predictability (as exemplified by the Laplacian demon, who could calculate the whole future of the universe if he knew the initial conditions of all the bodies in it).

With the advent of the quantum theory, it became clear that perfect prediction is impossible, if only because at the micro-level, entities such as electrons are not the sort of things that can have, simultaneously, a well defined position and a well defined momentum. Detailed predictive laws were replaced, as is well known, by statistical laws. Nevertheless, a strong effort was made to keep the *form* of facts, laws, and hypotheses as close as possible to that of the classical scheme. Thus, it was supposed that an electron is the sort of thing that can, at each moment, have a *quantum state*, determined by its wave function, $\psi(x, t)$. Some initial measurement gives us the quantum state at a given time, t_0, represented by the wave function $\psi(x, t_0)$. The law then takes the form of an equation (e.g. Schrödinger's equation or Dirac's equation) which predicts the future quantum state, and from which one can calculate future probabilities (i.e. $P(x) = \psi^*(x)\,\psi(x)$). In principle, one retained the ideal of determining the quantum state of the system at a given time, and of predicting the future of this state. As with classical mechanics, such predictions then afford a test of the theory.

It is clear that because a certain part of the general classical scheme for expressing facts, laws, and hypotheses has been retained, one still tends to regard prediction as the essence of physical theory. But is this really so? In order to define the question more sharply, we may ask whether it is essential that we first obtain our facts on the basis of a measurement of initial conditions, *then* make our calculations on the basis of laws or hypotheses, and *finally* make observations to check these calculations. Would it not be equally valid if we were presented all at once with a set of data taken at different times on the same system? Then we could consider the data corresponding to one time, t_1, calculate what to expect for a later time, t_2, and compare with the actual data. Surely such a test is just as good as one in which we do our calculations before we obtain the data for the later time, t_2. Vice versa, we could also take the data for t_2 and *retrodict* the conditions at t_1. This too would be just as good a check on the hypotheses. But equally well, we might consider ' mixed data ' taken from two times, t_1 and t_2, and see whether the remainder of the data taken either earlier, later, or in times that are between t_1 and t_2, are connected in the way implied by the hypotheses that are being tested.

If one reflects a bit, one will see that in modern high energy physics, most of the data is of the type described above, in which prediction is essentially irrelevant. For example, in cosmic ray research, a high energy particle may come in (at a speed close to that of light) and activate a counter, C_1. It then passes through a block of lead, several particles emerge, and a number of counters, C_2, C_3, C_4, etc., on the other side of the block of lead are activated. It would not only be unnecessary but impossible in practice for the physicist quickly to become cognizant that the counter, C_1, had been activated, then immediately to calculate what was going to happen while the particle was in the process of passing through the block of lead, and then to verify his predictions, by seeing whether the counters C_2, C_3, C_4, etc., clicked or not. What happens instead is that he is presented with a final result, in which connected events occurring at a series of two or more times are all recorded together, and he studies the relationships among these events to see whether they are as his theory says they should be.

It is clear then that the stress on prediction of future events in physics is not, in general, really justifiable. For we are frequently dealing with systems (especially with quantum mechanics and high energy physics) which are unpredictable and in which prediction is not the essential point. What is essential, quite generally, is that our theory should furnish us with a correct knowledge of the relationships in the system under investigation.[1]

The habit of thought by which one identifies all lawful relationships with predictability is capable of having an important limiting influence on the directions in which scientific research can develop. For it tends to blind us to the possibility that there may be real relationships in time having no fundamental connection with predictions. As a result, there may be a large range of facts that we simply do not look for, because there is no room in our general framework of thinking to express these facts.

In order to exemplify this point in more detail, let us return to our discussion of research on high energy particles. In certain cases, where such particles from outer space are incident on a photographic plate, there is produced a very long and highly ramified chain of particles, which is called a shower. Such a shower results from a series of collisions with the atoms of the plate, in each of which two or more particles emerge. Although events are produced in a series of collisions, as described above, we are, of course, presented with the whole of the data all at once, in the form of a set of tracks which the particles leave in the photographic plate.

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[1] H. L. Armstrong, in a private communication, has made the very pertinent comparison of the rôle of predictions in scientific research to that of examinations in university studies. In a cursory view, it might seem that students frequently act as if the main purpose of their work is to pass examinations. A more careful consideration shows, however, that the purpose of the examinations is to test for how well the student knows the subject. Similarly, predictions help us to test for how well we know the relationships in Nature that we are investigating.

Now, according to current views on predictive laws, we would use the quantum mechanics to predict the probability of the first collision. We would then consider a wave function representing the actual state of those particles that emerged from this collision, and with this, we would predict the probability of the next collision. A similar procedure would then be carried out on these particles, and in this way, we could eventually discuss the development of the whole shower in a chain of collisions.

In such a treatment, the only facts that we could take into account would be statistical correlations, such as that *A* is followed by *B* in a certain fraction of the cases, *B* by *C*, etc. As we have seen, however, we could equally well turn the problem around, and treat retrodictive relations. Thus, in a certain fraction of cases, *D* came from *C*, *C* from *B*, etc. But what is even more important is that there could exist over-all features of the pattern of events, which could not be expressed solely in terms of the order of succession of events or of its inverse. To see what could be meant by such features, consider, for example, a distribution of objects on a line in space. If the distribution has a certain kind of simple repetitive pattern, then one could predict the objects to the right of a given point if one knew what was to the left and vice versa. However, more generally it is evident that there could exist quite definite and regular relationships which would not allow such a prediction. We have a similar experience with order in time in a musical composition, in which the whole pattern of notes may be quite definite, while, nevertheless, a given note is not determined completely by those which came before it. What we are proposing is that the fundamental laws of physics may have such features too. And if this should be the case, then they would imply the possibility of new kinds of facts; namely, relationships that cannot be expressed in terms of serial order alone. Facts of this kind must escape our conceptual net as long as we think only in terms of predictions of future events on the basis of initial conditions.

Naturally, if such new forms of relationship in time exist, they will require new theoretical schemes to express them. The whole idea of writing a wave function at a given time and calculating a future wave function with a Hamiltonian would have to be given up (except as an approximation valid in the limiting case of atomic orders of dimensions, for which the present theory is evidently essentially correct). One would have to come to different conceptions concerning order and relationship in space and time, which evidently cannot be discussed in detail in an article of this nature. However, it can be said that such changes in concept are quite consistent with the general character of the quantum theory and would indeed lead to a more natural expression of them than is now possible.[1]

We see then that it may well be important to get out of the habit of thinking that in the expression of the fundamental laws of physics, all facts must take the form of initial conditions referring to a given moment of time, while all laws must take the form of relationships, permitting the prediction of events at a later moment of time.

We now come to the second general habit of thought that was to be discussed here; namely, specialisation. Of course, it is evident that specialisation has been forced on us by the overwhelmingly large mass of complex data available today. It is generally realised that while specialisation has important advantages, it requires us to pay a price to get them. There is, however, a certain aspect of this price, that does not seem to have been considered adequately. This is that specialisation narrows our conceptual framework sufficiently so that we cannot even express facts of a certain kind; namely, those whose framework essentially and intrinsically cuts across the boundaries between various fields. (Here, one is referring to facts of a kind that cannot occur in ' border-line ' sciences, which are essentially fields of specialisation that overlap two or more other fields.) If such facts exist and are important, then it may well be that the price of specialisation will eventually involve the erection of a barrier to the very knowledge that we expect to get through it.

To bring out the point in more detail, let us consider, for example, the relationship between physics, chemistry, and biology. Of course, each of these sciences studies its problems in its own specialised way, and it is generally admitted that this is necessary, at least to start with. But by now, there is a growing realisation that these subjects are closely related. The most common form that has been suggested for this relationship is that physics is the ' fundamental ' science, which could, in principle, contain the other two, if only we knew its laws deeply enough and could calculate their consequences with sufficient detail.

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[1] The author expects to publish some preliminary results of a study of this question in the near future.

In other words, it is supposed that all the facts that the chemist and the biologist deal with could, in principle, be expressed in terms of the concepts of physics (even if this might prove to be so impracticable that it would never really be done in full detail), while none of the facts of physics would ever require the concepts of chemistry or biology for their expression.

If we reflect on this point of view for a while, we will see that it is very similar to several others that we have discussed here. Nineteenth-century physicists thought that all facts could be expressed in terms of Newtonian concepts. Before the hypothesis of non-conservation of parity was considered, it was thought that all facts could be expressed in terms of the concept of the symmetry of space. Even now, physicists generally proceed as if all facts can be expressed in terms of some kinds of initial conditions (although a tendency toward developing other points of view has begun).

The notion that everything is, in principle, reducible to physics has in common with their various points of view the character of being an unproved assumption, which is capable of limiting our thinking in such a way that we are blinded to the possibility of whole new classes of fact and law.

In order to make this point in a still more definite way, let us consider the study of living systems in biology. Now living systems are characterised by a great complexity of organisation and a high degree of integration. It may well be that atomic particles out of which such an organised and integrated system is constituted follow laws slightly different from those obtained by extrapolating the laws applying in systems consisting of only a few particles. These differences might be so small that it would be extremely difficult to detect them except in their over-all cumulative effects in a living organism.

If such were the character of life, then it is clear that the hope of expressing all the laws of nature *solely* in terms of physical concepts could not be fulfilled. In other words, it is possible not only that the concepts arising in the physicist's field of specialisation are not sufficient for a complete treatment of all aspects of Nature, but also, that these concepts may not even be completely adequate for relating all the facts that might arise in the physicist's own field.

The above is not a purely abstract possibility. Indeed, there are specific reasons why it may actually be relevant. For example, if we consider the modern quantum theory, we note that it does not deduce classical theory as a limit, in the same way that relativity theory leads to non-relativistic theory, when the velocity is small compared with that of light. Rather, quantum theory *presupposes* the applicability of classical theory.

At first sight one might think that the above statement is contrary to the results of certain approximational techniques (e.g. the Wentzel-Kramers-Brillouin), which can be applied, for example, to Schrödinger's equation, to show that with heavy objects, the spread of a wave packet is so small that it can, in general, be neglected for practical purposes. Thus, one would expect to be able to represent a ' classical ' object by a wave packet of negligible width, which does not spread appreciably in any period of time that is likely to be of interest.

It must be remembered, however, that there is an important difference between an approximation which is good enough for a certain limited range of practical purposes, and a correct *logical* formulation of the theory. It is well known that as conditions and context change, even what are initially small errors can lead to qualitatively new conclusions. And indeed, there are many conditions and contexts in which the representation of classical objects by wave packets can be shown to be inadmissible, especially those in which it is significant that the wave function generally is multidimensional. An important case of this kind arises in the quantum mechanical description of the process of measurement itself. The observing apparatus is a large-scale and essentially classical system. We consider an experiment in which this apparatus is used to measure some property of a micro-system to a quantum-mechanical level of accuracy. Then, after the process of interaction between the apparatus and observed system is finished, but before any observer has looked at the apparatus to see what the result is, the wave function for the combined system takes the form of a linear combination of packets, covering all the possible results of the measurement. On the other hand, we know that in reality, the combined system is in some *one* of its possible states, and that the observer's act of looking makes no significant change in the state of this apparatus (just because the latter is so heavy as to be essentially classical). Thus, here is a case in which the actual state of a large-scale system cannot be represented by means of a single narrow wave packet, because even the wave function of the large-scale system can cover a significantly ambiguous range of possibilities, when it has interacted with a micro-system in such a way as to make a measurement of the properties of the latter possible. (A similar result could follow for a wide range of interactions with a micro-system which were not measurements at all.) It is clear that classical systems have certain properties that cannot always be represented by narrow wave packets which do not spread appreciably.[1]

Roughly speaking, it may be said that the quantum theoretical concepts imply a certain range of potentialities (described by the wave function) as well as a tendency for each system to cover all of its potentialities. On the other hand, classical concepts express the possibility of a system to settle down to one actual mode of behaviour; that is, to choose one actuality out of all these potentialities. In this way, classical concepts make an essential contribution to the expression of the total content of the theory, which is not reducible to an approximation to what appears on the quantum-mechanical side alone. The correspondence principle then merely asserts the compatibility of the quantum theory with the classical theory in the large-scale limit.

Now, it is clear from the above that the classical theory by itself can be accepted as a good approximation in large heavy systems containing many particles.[2] Suppose, however, that we consider an intermediate domain, in which systems contain a fairly large number of particles, not large enough to be fully classical in their behaviour, and yet large enough to *begin* to exhibit the classical tendency to settle down to a definite mode of actualising its potentialities. In the intermediate domain, there is room for a new kind of law, which in some way synthesises the classical and quantum-mechanical sides. Such a law could not have shown up as yet in our previous studies, in which careful and precise investigations have really been made only of the laws of systems consisting of a few particles.

Now, it is interesting that this intermediate domain is just about that of the order of size of virus particles, which are on the borderline of life. It is well known that these are essentially large molecules, having some quantum mechanical properties, and yet coming close to the classical limit. A law combining classical and quantum properties would express something analogous to one of the peculiar and characteristic properties of living systems; namely, to have a tremendous range of potentialities and to be continually moving toward various actualisations of these potentialities, without thereby falling into a state in which they have no further potentialities.

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[1] This whole question is discussed in detail in D. Bohm, *Quantum Theory*, New York, 1951, Chaps. 22 and 23.

[2] Provided, of course, that these are not interacting to a significant extent with essentially quantum mechanical systems, in the way described previously in connection with a measurement process.

If such laws exist, then there could be facts that do not fit into the physicist's frame of concepts. They might well be expressible *fully* only with the aid of new concepts which are, in effect, a definition of some of the essential characteristics of living systems.

The above discussion shows in some detail that there is a distinct possibility that the current tendency toward specialisation may defeat its own purpose. It is true that by specialisation we can probably accumulate more and more knowledge of a certain kind; namely, details appropriate to the various fields. But by doing this, we may be leading ourselves away from another category of knowledge, a category that may be far more interesting and more important for our general welfare.

To sum up this talk, we wish to call attention to the relationship between the methods of scientific research, and the content of scientific knowledge. The method must be tailored to the content; and if one loses sight of this, one is in danger of being artificially limited in a way that easily escapes conscious realisation. Method is determined in part by the effort to ask relevant questions in our researches; and it is essential to understand that the relevance of a question depends on the character of the material under investigation. Such questions help determine the forms of the facts that can be elicited in further researches. These questions are, in general, limited firstly by our concepts, laws, and hypotheses, and secondly, in a less obvious but equally important way, by our general habits of thought. Such habits can easily blind us to the need for altering our ways of thinking in accordance with the nature of the material under investigation as we penetrate into new domains.

In order to avoid the dangers that have been discussed here, it would seem that it is desirable to re-establish what was once called a 'natural philosophy'. Of course, it is clear that such an attempt would run against the general current as it is today in science. Nevertheless, it will, in all probability, eventually be necessary, if the full potentialities of the scientific method are to be realized.

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COMMUNICATIONAL EPISTEMOLOGY (III) *

MAGOROH MARUYAMA

3 *Limitations of Interpersonal Understanding*

WE consider the limitations of interpersonal understanding under three headings: logical limitations, information-theoretical limitations, and resonances.

3.1 *Logical limitations.* The completeness and closedness of a theory (or interpretation, thinking pattern, etc.) give a natural boundary to the theory. Completeness and closedness are defined as follows:—

A theory T is complete if and only if every sentence constructed with the symbolism of T according to the rules for handling it or its negation is in T.

A theory T is closed if and only if the results of all operations given in T on all elements of T are in T.

Here a ' theory ' need not be a deductive theory. And operations are regarded as being inherent in the theory. Instead of stating: ' The set is closed under the operation P . . . ' we state: ' The set with the operation P is closed. . . . ' The reason is that a culture includes reasoning patterns rather than reasoning patterns operating from outside on cultural elements.

In a complete theory any question constructed by grammatical combinations of words found in the theory can be answered by yes or no except the questions starting with what, how, who, etc. But questions such as ' Who did this? ' can be replaced by a set of questions ' Did A do this? ', ' Did B do this? ', etc., as long as the range of ' who ' is included in the theory. As long as no new word or concept is introduced, the theory does not have to expand to include new elements in order to be complete. A complete interpretation is self-sufficient, so to speak. And when one is inside a complete interpretation, its self-sufficiency makes one think that it is the only and correct interpretation. But completeness of an interpretation does not of course guarantee its correctness.

A closed system, on the other hand, does not generate anything new unless new elements or new operations are added. For example, the set of positive integers together with the operation of adding and multiplying is a closed system because the result of adding or multiplying any two positive integers is again a positive integer. But if a new operation 'subtraction' or a new number -1 (or any other negative number) is given, the whole series of negative integers and zero is generated. (Subtraction can be generated by negative integers and addition.) Then the system of positive integers, negative integers and zero together with addition, multiplication, and subtraction is again closed. It cannot be enlarged again until a new element such as a fractional number or an imaginary integer, or a new operation such as division or extraction of square roots is introduced.

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* Parts I and II appeared in the preceding two numbers.

The system of communicable reality is likewise closed. Suppose someone believes that the universe is the universe of communicable realities. What he does in his activities is to start with communicable entities and then manipulate them and combine them, using the operations which are in his universe, i.e. those operations and their results which are definable in communicable terms. The products of his activities are again communicable. In his system he never encounters anything incommunicable, nor does he need anything incommunicable. He lets in no contradiction by rejecting everything incommunicable.[1]

Western rational thinking, based on the universe of communicable entities, lets in no contradiction by not considering incommunicable or irrational realities. But it would get contradictions if it took them into consideration. It is a closed system. In order to enlarge this closed system, we have to introduce something from outside the system. In fact, we need not go to non-Western cultures to find a new element. In Western cultures, psycho-analysis and psychiatry as well as poetry have found the existence of incommunicable realities such as the unconscious. Philosophy may keep a respectful distance from them. But their existence makes it no longer possible to deny the fact that the rational system is closed. This awareness of the rational system (or the system which is called 'rational' in Western cultures) as a closed system may logically eliminate some of the prejudices against non-Aristotelian logics as 'illogical'.

A question may be raised here as to the possibility of full communication between two different thinking patterns starting from the intersection (i.e. the common parts) of the thinking patterns, provided that the intersection is not empty. The answer is that, if the two thinking patterns are closed systems, their intersection is also closed, and therefore by starting from nothing but the elements and operations given in the intersection, nothing outside the intersection is gained, and consequently the parts which are outside the intersection remain incommunicable to each other. Let A and B be the two closed systems and D their non-empty intersection. Then the elements and operations in D satisfy two conditions: (*a*) they are in A, and (*b*) they are in B. By virtue of (*a*) whatever they can generate is again in A; and, by virtue of (*b*), whatever they generate is again in B. Therefore, whatever they can generate is again in D.

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[1] See Part I, first reference

When two persons, one having a closed and the other an open thinking system, try to start from their common ground, the chances are that the person with the open system will go beyond the common ground and get into the other person's thinking outside the common ground, while the person with the closed system will remain within his system. The intersection is half-open, i.e. it is open from the open to the closed side but closed from the closed to the open side. For example: Let a person A have a closed system of the universe of communicable entities, and another person B an open system of some incommunicable entities and some communicable entities. (We can think of A as a Western philosopher and of B as a Chinese poet, for example.) As A's thinking is based on communicable entities, with enough time and effort A may succeed in educating B and making B learn A's thinking by communicating with him. But even after B has learned A's thinking, the incommunicable part of B remains incommunicable to A. (As we shall see later, incommunicable realities can be 'resonated' between individuals having the same realities. But since A has no incommunicable reality he cannot resonate with B's incommunicable reality.)

Thus a person with a closed thought system cannot understand anything beyond his system by manipulating the elements and the operations given in his system. Only by accepting elements or operations from outside his system can he go beyond his closed boundary. On the other hand, a person with an open thought system may generate new elements and operations by manipulating the elements and operations within his thinking system. But this does not mean that he can generate every element and every operation found in other systems. As we have seen, the system of all positive integers and one negative integer, together with addition and multiplication, is open. It can generate all negative integers, zero, and subtraction. But after having generated them it becomes a closed system; it does not generate fractions, imaginary numbers, division or the extraction of square roots.

Thus, an open thought system cannot necessarily expand itself to embrace a different thought system. The surest way to expansion is to introduce the elements and the operations found in the second system to the first system, even if the first system is open. Willingness to accept unfamiliar elements and operations which cannot be accounted for in one's own thought system is necessary for the growth of any thought system.

Another hint we get from mathematics is the concept of analytic continuity in the theory of functions of complex variables. It concerns the representation of a function by a power series. The representation by a power series (Taylor's expansion) of an analytic function in complex variables cannot go beyond the domain of convergence of the series. By choosing within the domain near the boundary of convergence another point of expansion and then constructing another power series one may continue the representation of the function beyond the domain of convergence of the first power series. This process may be continued to fill a connected region within which the function is analytic.

In other words, a power series covers only part of the region of the function. But by choosing another centre for another power series near the boundary of the convergence of the first power series but within the boundary, one may cover a part beyond that boundary. The two power series represent the function in different parts with some overlapping, and they coincide where their domains overlap. By repeating the process we can cover a whole connected region within which the function is analytic.

Similarly, it may be possible to cover a region of thought by a set of interpretations, each of which is not enough to cover the whole region, and each of which originates from another to form a lattice.

A question which may arise from our discussion of the logical limitations of interpersonal understanding is: 'As human nature is everywhere the same, is not anyone able to understand any other person's thinking pattern?' Our reply to this question follows.

A large part of our 'thinking pattern' is formed in infancy in the pre-logical and pre-scientific stage and is later reinforced by our childhood experience in the culture in which we grow up. And so to think like a Balinese, for example, we have to divest ourselves of our rational, 'logical' mode of thinking, return to our pre-scientific and prelogical mode of thinking, and begin again along the Balinese line of personality development. But the process of personality development is largely irreversible. Though we are all born with the same human nature, we are reared in different cultures and become different; the differences are never completely overcome by 'communication'. We may describe the behaviour of the Balinese with dimension reduction and projection, but this is not 'understanding' the Balinese pattern of thinking. We cannot attain *tabula rasa.* An electronic brain can attain *tabula rasa* by erasing all its stored information, but it has a specific logic built into it and can never attain the pre-logical stage.

3·2 *Information-theoretical limitation.* Here a knowledge of the basic ideas of information theory is assumed. The internal state of the transmitter T is encoded into a 'message' which consists of verbal and nonverbal, conscious and unconscious behaviour of T. When this message reaches the receiver R, and before it is decoded by R, there is some loss of information and some distortion. The loss of information is caused by such factors as language difficulty, unfamiliarity of R with T's behavioural pattern, aspects of the message which escape R's attention because of cultural or personality differences between R and T, etc.; all these losses fall under the category of 'noise'. On the other hand, such distortions as social perception, projection, dimension reduction, may be considered either as noise or as a part of decoding. It is tempting to define non-singular (reversible) distortion as decoding, and singular distortion as noise. But the distinction cannot be applied in our case because decoding functions and their inverse functions do not have to be univalent functions.

Let x be T's internal state, f be T's coding function, w be $f(x)$ or T's behaviour, z be the part of w which reaches R after interference by noise, g^{-1} be R's decoding function, and y be $g^{-1}(z)$ or R's interpretation of z. Then the quantity K, where

$$\begin{aligned} K &= H(w) - H_z(w) = H(z) - Hw(z) \\ &= H(z) + H(w) - H(z, w) \end{aligned}$$

H designating entropy,

is meaningful as a rate of communication only if f, g and their inverse functions are univalent, giving $H(x) = H(w)$, $H(y) = H(z)$, $H(x,y) = H(w,z)$, and only if f is known to R and g is known to T. When these conditions are satisfied, w and z in the equation can be replaced by x and y respectively. But as these conditions are usually not satisfied, the quantity K is meaningless. Nevertheless we can say that equivocation results from 'noise' and from an excessive rate of communication, i.e. if there is noise in the communication system, or if the rate of information transmitted exceeds the capacity of the receiver, then equivocation occurs in R. Thus the capacity of the receiver sets a limit to the rate of interpersonal communication.

Another important limiting factor is the background information. In interpersonal interaction there is usually a body of common background information in the light of which a new message makes full sense. For example, if in the American culture someone distributes cigars in his office, one knows, because of knowledge about the custom in the culture, that a baby has been born to that person, that he must be happy and busy, that his wife needs help, that he may not sleep well, and so on. Or such background meta-information as 'A tends to make pessimistic statements', or 'B does not usually make insinuations' helps one to evaluate the message adequately.

Mathematically, the gain of information from a message is proportional (or is additive on a logarithmic scale) to the relevant background information if there is no redundancy between the message and the background information. In other words, if no part of the message overlaps with what one can infer from one's background information then doubling the amount of relevant background information doubles the amount of information one gets out of the same message. The informative gain of the message is not determined by the structure of the information bound in the message alone, but also depends on the background information of the receiver.

For example, a professor of chemistry and one of his students may both read a report on a new chemical substance which they have not known of before. Both get the same information from the report. (In chemistry there is no such thing as a nuance of interpretation as there may be in reading poetry.) But the professor, having more relevant background information than the student, can extract from the same report some additional information; he may the more fully appreciate the ingenuity of the methods used in preparing the substance, the applicability of the method to other preparations, the competence of the reporting chemist, and so on. The student is not yet capable of extracting all this information from the same report. On the other hand, if there is redundancy between the report and the background information of the professor, i.e. if the professor could infer from his background information a part or whole of the report, the information value of the report to the professor decreases because of the redundancy. The student, whose little background information has no redundancy with the report, may gain more information from the report than the professor.

The importance of background information becomes apparent when a visitor arrives in a foreign culture in which he has no background information about the conventions, the speech, and behavioural patterns of the people. He bitterly realises that everyone is an unwitting expert in the way of life of his own culture. And his unconscious and unwitting assumption of the pattern of thinking in his own culture, so basic to him that he never notices its existence, can be more harmful to him than ignorance. In such a case, the ‘ wrong ’ background information functions as noise or interference in communication. Thus, the absence of the necessary background information and the presence of ‘ wrong ’ background information limit interpersonal understanding.

3.3 *Resonance.* Here it is proposed to consider, very briefly, only five distinguishable types of resonance, though there may well be others.

Intellectual resonance is resonance dependent upon capacity of perception, of decoding and internal process as well as the background information of the receiver. When the message exceeds the capacity of the receiver, equivocation and loss of information result. As we have seen, the information gained from a message depends on the amount of the background information and its relevance and redundancy with respect to the message. When the communication is properly matched to the capacity of the receiver, i.e. the rate of information input is neither too great nor too small, he is said to be in optimal intellectual resonance with respect to the communication.

When two persons derive the same information from a message (either between each other or from another source), they are said to be in intellectual mutual resonance with respect to the information bound in the message.

The distinguishing characteristic of intellectual resonance is that it depends only on the capacity of the receiver (including his perception, decoding process, background knowledge and internal process). The capacity of the receiver, especially his background information, can be improved by intellectual process. On the other hand, the transmitter can reduce his rate of transmission. Thus between two intelligent persons intellectual mutual resonance can be achieved, at least theoretically, with time and effort. (They do not have to agree with each other's opinions; each has only to understand what the other means.)

Meta-intellectual resonance is best explained by an example. The

Balinese language, spoken in the Bali culture of Indonesia, conspicuously lacks abstract general words. Each event needs a new vocabulary. When a canoe is to be built, the builders first work out the names for 200 parts of the canoe. When the canoe has been completed the builders forget the vocabulary. At the next canoe construction the builders have to work out another set of 200 words which has no relation to the first set of 200 words. Each event requires a new vocabulary, and the vocabulary for an old event has to be forgotten as fast as possible so that the memory can be used again for a new vocabulary. There are words for ' to cut in even slices across the loaf', ' to cut irregularly ', ' to cut in asymmetric pieces sideways ', and about thirty different ways of cutting a loaf of bread. They are distinguished strictly, and if the transmitter uses a close but wrong word, the receiver is not able to guess what is meant.[1]

On the other hand, the Manus language, spoken in a culture in the Admiralty Islands in the South Pacific, is a highly analytical language. There are no figures of speech, no gender, only third-person pronouns, and very few adjectives. It is a cold, accurate, precise language of people who can count to 400,000 without any form of notation. In short, it is close to the language of mathematical logic.

Now we may ask: Can a Balinese communicate with a Manus without difficulty ? The answer is obviously negative. The difference between the thinking pattern of the Balinese and that of the Manus is not a linguistic difference which can be bridged by translation. The difference lies in the level of abstract thinking. It is a meta-intellectual difference. If two persons are on the same meta-intellectual level, we say they are in meta-intellectual resonance. Differences in meta-intellectual level limit interpersonal communication.

Modal resonance. Modal resonance occurs between people using the same mode of communication—denotative, connotative, situation-contextual, verbal, kinesic, somatic, etc.

In some cultures, such as the Swedish, the denotative mode of communication prevails. Expression (encoding) and interpretation (decoding) are straight-forward. In some other cultures, such as the Danish, connotative communication, with indirect expression and insinuation, prevails. The encoding may have a set pattern which is known to all those in the culture. In such a case decoding is accomplished without misunderstanding. The complicated rules of encoding may become internalised, i.e. inseparable from the thinking process to such an extent that the processes of encoding and decoding become automatic, unconscious; those using it believe that it is the most natural, direct mode of communication and are not able to communicate in other modes. If a person in a culture with this mode of communication receives a message from a person from a culture with denotative communication, the receiver automatically and unwittingly decodes the message according to his own rules of decoding and finds meanings which were not intended. Similarly two persons with different types of connotative communication will read wrong meanings in each other's messages. On the other hand, the meaning a connotative transmitter puts in his message is not properly decoded by a denotative receiver. In all these cases, people may accuse eath other of being unintelligent, illogical, unclear, etc. The incorrect decoding due to the differences in the types of connotativity may be considered as a variation of projection. But it may be differentiated from projection; projection is the addition of extra dimensions in the interpretation, and the dimensions added are usually those with which one is preoccupied and are therefore rather psychological. But incorrect decoding which is due to differences in types of connotativity adds no extra dimension to the interpretation. It is analogous to using a wrong key in cryptography. It is culturally institutionalised while projection is individual. But the borderline between the two cannot be drawn sharply, especially because psychological preoccupations may become culturally institutionalised as a mode of communication.[1]

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[1] *Transactions of the 7th Conference on Cybernetics*, Josiah Macy Jr. Foundation, 1950, pp. 174-175

In contrast with the connotative communication in the Danish culture, which is unambiguously decodable among the Danes, some modes of communication in the Japanese culture are not decodable even among the Japanese. Different internal states may be coded into the same form of behaviour, e.g. into a smile. It is concealment if not deception. (Westerners sometimes report that a Japanese would tell his company what he believes would please them, that he does not tell them his true opinion. But this interpretation is incorrect. A Japanese identifies himself with the social institution or situation in which he finds himself. When he is with a superior or a guest, he identifies himself with the other's opinion, and even temporarily believes it. The Japanese has no continuous individual identity, but lives as a 'social identity' which may change from situation to situation.[1] This does not imply that there is no deceptive communication in Japan. There is much deceptive concealment of shortcomings to 'keep face'.)

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[1] M. Maruyama, 'The Multilateral Mutual Simultaneous Causal Relationships between the Modes of Communication, Sociometric Pattern and the Intellectual Orientation in the Danish Culture', Mimeographed, 1959; 'The Dane's Concept of Superficiality, Childishness and Generalization', Mimeographed, 1958; 'The Danish Language', Mimeographed, 1957; 'A Test in Intercultural Communication', Mimeographed, 1959; 'The Images of the Swedes', Mimeographed, 1957

A good example of situation-contextual communication is found in the Danish culture. In this mode of communication, the encoding and decoding do not depend on the structure of the message but rather on the situational context within which the message is sent. For example, a Dane may say 'I like it' and mean either 'I like it' or 'I do not like it' depending on the situational context. In order to decode the remark correctly one has to know the situational context including his taste and attitudes. But if one knows the situational context well enough to decode his remark properly, the meaning of the remark is known without the reception of the remark. In this case, verbal or non-verbal expressions cease to be a primary means of communication. People communicate through situational contexts. It may also be remarked that Danes are more interested in reading the state of mind of the sender of the message than in the contents of the message.

Again, in some cases, apparent communications do not have communicative purposes. An insult may not be intended to insult the receiver of the message: it may serve merely as an outlet for the sender's frustration. In the Danish culture, when the person insulted does not understand the insult (which is usually expressed indirectly) the insulter is nevertheless satisfied with the feeling that the insulted person is not perspicacious enough to understand the insult and is therefore inferior to the insulter. The satisfaction of the insulter is still greater if there is a third person present who understands the insult while the person for whom the insult is intended does not. In this way insults can be made which would imperil the insulter if the insulted understood the insult. Many of the practical jokes played by the Danes on German soldiers during the German occupation of Denmark were of this kind. The Germans could not make 'head or tail' of the jokes ; they did not even realise that they were being insulted. The Danes were delighted.

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[1] M. Maruyama, 'Air-sucking as Regressive Oral Gratification in the Japanese Culture', *Review and Newsletter*, Section of Transcultural Psychiatry, McGill University, Montreal, **6**, 12-13

In some other cultures, messages are deceptive and are expected to be deceptive. In such a culture those who conform with the conventions of deceptive communication are not deceptive. On the other hand, a foreigner with a differently deceptive or even non-deceptive mode of communication is deceptive because he does not conform with the conventions of communication within the culture. And when a person in such a culture gives a foreigner a deceptive message expecting him to figure it out, or at least to be prepared for a deceptive message, and the foreigner does not even suspect that the message is deceptive, then the foreigner misunderstands the sender of the message as well as his expectation, and the sender deceives the foreigner unwittingly. Thus, persons with differing deceptivities (including absence of deceptivity) misunderstand each other and accuse each other of being deceptive and dishonest.[1]

In other instances messages have no communicative but only a vague suggestive function. In Japanese poetry every preciseness is avoided. A vague theme and an atmosphere may be suggested in very few words, and nothing more. It is the reader's task to interpret it. The more ambiguous the suggestion and the more numerous the possible interpretations the better the poem. The ego is all-pervasive and dissolves in the atmosphere. But in Western poetry a precision of feelings and emotion is evoked. The ego acts towards, opposes, distinguishes itself from its environment and other individuals.

A normal adult has learned to use verbal communication, kinesic communication (through behaviour), practic communication (through action and performance), and somatic communication (through physiological manifestations). These modes of communication develop at different stages of a person's development. If, at the time of its development, a mode of communication attracts no satisfactory acknowledgment from others or gives no gratification to the person exercising it, the person will later tend to avoid this mode. If two persons rely chiefly on different modes of communication they will find mutual understanding difficult to attain.[2]

Experiential resonance. Some realities can be understood only by living through them. Among them are certain states of happiness, certain degrees of seriousness, certain senses of humour, aspirations for freedom, the spirit of the Hungarian revolution, and suicidal despair.

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[1] P. Linebarger, *Psychological Warfare*, 1955. Also E. W. Barrett, *Truth is Our Weapon*, 1953; also Part I, second reference

[2] J. Ruesch, *Disturbed Communication*, 1957, 80

Such realities cannot be communicated directly, but can be resonated, or triggered by some cues, between persons who have experienced them. These experiences are objective in the sense that they are real to those who experience them and that they can be shared by many persons. They are not objective in the sense that they cannot be communicated to all persons, and that they cannot be described scientifically.

Relevance resonance. Communication fails if a message, considered important by the sender, is discarded as irrelevant by the receiver. Relevance resonance is similar to intellectual resonance in that it requires some common intellectual background between the communicating persons. It differs in that it does not require exact matching of intellectual capacity and that it requires similarity of interests, goals, and points of view. Further, relevance resonance is related to experiential resonance; if there is experiential resonance between two persons, it is likely that this will result in relevance resonance (but not vice versa). Two persons who have suffered from the same kind of difficulty, extreme hunger or suicidal despair, or who have participated in some intensely emotional experience, have an experiential resonance between them; this sameness of experience may well lead to a sameness of interest, goal, and point of view and thus result in relevance resonance. This example may suggest that experiential resonance is related mainly to the past and relevance resonance to the future. But experiential resonance can be related to anticipation and relevance resonance to past experience.

4 *Concluding Remarks*

We have now completed an introductory survey of the three stages of our inquiry into communicational epistemology. For illustrative purposes we constructed simple models and discussed them formally rather than empirically. The formalisation served to reduce the treatment of the problem to precise logical operations and to emphasise that communicational epistemology can be treated in terms of algebra, operators, and functions, i.e. can be encoded into logical and mathematical terms in order to communicate with the philosophers, logicians and mathematicians who think mainly in these terms. But we have to remind ourselves that an epistemological study has to build not only its contents but also its form on empirical data. Traditionally it has been thought that the form of human knowledge could be determined *a priori* without considering empirical data. We do not deny the existence of some form of human knowledge determinable *a priori*, but we have to realise that much of the form considered in the past to be determinable *a priori* has turned out to be an *a posteriori* internalised process, e.g. the action-state dichotomy, the subject-predicate relationship. Empirical studies are therefore indispensable both for acquiring the content of epistemology and for determining the boundaries of its *a priori* form.

When constructing a formalised model, we should always have the empirical situation in mind. It is the empirical situation which suggests the model. A model is necessarily an oversimplification of the empirical situation. But by having the empirical situation in mind, one knows exactly where and how it is oversimplified, and consequently where and how it can be applied. On the other hand, a model constructed with little or no empirical study remains on the level of oversimplification, and the constructor is unable to determine where and how it can be applied. To arouse interest in the theory we have presented here a discussion with little empirical material to support it. But the direction of the study of communicational epistemology should be from intensive empirical studies to theoretical formulations. Some of the results of empirical studies have been published.[1]

The present discussion has been based on the assumption that there are cultures with different thinking patterns, encoding and decoding functions; a summary of some observed differences follows.

Examples of contrasted patterns of thinking, encoding, and decoding functions.

(1) The communication pattern in the Danish culture contrasts sharply with that of most of the Western cultures. Expressions of one's opinions and feelings are considered to be impolite, aggressive, and childish. It is likewise considered aggressive and impolite to ask others about their opinions, feelings, and professional interests. Good conversation consists in manipulating trifling matters of daily life with humour. Introspection, projection, and subjectivism are mature forms of thinking while the accumulation of facts and objective knowledge and the expression of opinions belong to adolescence and are therefore regarded as immature and superficial. Communication is mainly connotative and situation-contextual. The Danish culture provides a sharp contrast with the intensive intellectual

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[1] See Part I, second note

intercourse of the French and German cultures, with the accumulation of universal knowledge and articulate expression in France, with thoroughness and systematisation in Germany, and with the denotative mode of communication prevailing in Sweden. In Denmark a respect for isolation and non-interference is a form of considerateness, and this attitude interrupts and reduces communication, especially with reserved new acquaintances and withdrawn individuals.

(2) In the Chinese pattern of thinking there is no concept of substance, and the subject-predicate relationship is dispensible.[1]

(3) Hopi metaphysics is based on the distinction between what is manifest, or accessible to the senses, and what is not. The manifest includes experienced events in the past and present with no distinction between past and present. The ' unmanifest ' includes future events, secondhand information (past or present) and all that is mental. As an event at a distant place is heard of only after a period of time has elapsed, the more distant the event the more remote in time it is. There is a clear distinction between myth (which is mental) and reality (which is manifest).[2]

(4) To the Chichewa of East Africa the past recorded in memory is distinguished from the past recorded in the external physical world.[3]

(5) Cœur d'Alène distinguishes between growth (maturation of an inherent cause), addition from without, and something affected by addition from without.[4]

(6) There is a great difference between the levels of abstraction in the thinking patterns of the Balinese and the Manus.[5]

(7) English understatement appears arrogant to Americans; American overstatement appears boastful to the English.[6]

(8) The Manus find goals and security in activity, the Chinese in stability.[7]

(9) In a culture in which motives cannot readily be put into action, action is more superficial than motives. In a culture in which motive

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[1] T. S. Chang, ' A Chinese Philosopher's Theory of Knowledge ', *E T C.*, 1952, **9**, 203-262

[2] B. L. Whorf, *Language Thought and Reality*, 1956 [3] Ibid. [4] Ibid.

[5] *Transactions of the 7th Conference on Cybernetics*, 1950

[6] M. Mead, ' An Application of Anthropological Techniques to Cross-National Communication ', *Transactions of the New York Academy of Science*, 1947, series 2, **9**, 133-52

[7] M. Mead, *Co-operation and Competition among Primitive Peoples*, 1937; *New Lives for Old*, 1956

can readily be put into action, motives without actions are superficial.[1]

(10) In a culture in which concealed maliciousness is a real danger, failure to perceive it means wrong social perception. In a culture in which no such maliciousness exists, lack of suspiciousness is not a sign of naivety, as it is in the first culture, but is more realistic.[2]

(11) In the Arabic culture an emphasis seems to be given to detached details.[3]

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[1] M. Maruyama, 'Critique de quelques idées très répandues au sujet des rapports entre les cultures et la santé mentale', *Revue de psychologie des peuples*, 1959, 14, 273-6

[2] Ibid.

[3] M. Bleuler and R. Bleuler, 'Rorschach's Ink-blot Test and Racial Psychology: Mental Peculiarities of Moroccans', *Character and Personality*, 1935, 4, 97-114; *Memoirs of American Anthropological Association*, 1955, 81

NOTE ON THE AXIOMATIC FORMULATION OF ELECTROSTATICS* [1]

Raymond A. Santirocco

The axiomatic foundations of various branches of physical theory have received a good deal of attention in recent years, and it is generally considered of great pedagogical importance to emphasise to the student the conceptual framework within which a particular set of sense phenomena is to be described. Elementary electrostatics is an area where this objective is frequently overlooked. The purpose of this note is to give an axiomatic formulation of the foundations of elementary electrostatics, with emphasis on the empirical aspects of the new primitive signs and relations introduced into the formalism. We also give a proof of the uniqueness of the electrostatic field, making use of an often neglected superposition axiom of the theory.

1 *Introduction*

We shall follow the general lines of logico-empirical analysis in assuming that a theoretical system suitable for comparison with experience must contain three kinds of statements : equations, logical and mathematical rules, and statements relating the symbols in the equations with facts or objects of experience. We will label statements of the first kind either ' axioms ' or ' axiomatic definitions ', according to the conventional mathematical usage. Statements of the second kind—logical and mathematical rules—will largely be ignored, not because they lack importance or interest but because the chief aim of this paper is to exhibit the empirical content of a physical theory. We shall simply assume that sufficient logical and mathematical rules exist to define unambiguously and consistently all the manipulations we require of the formalism.

Statements of the third kind, relating the primitive elements of the formalism to the results of experience, are essential to any formal

* Received 3.v.59

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[1] The author is indebted to Dr E. C. G. Sudarshan for many valuable discussions on the topic of this paper and related topics.

system purporting to be a representation or description of sense phenomena. These statements have taken various forms—the operational definitions of Bridgman and the reduction to protocol sentences of Carnap, to mention just two. The operational definition with its connotation of specific experimental procedure is perhaps closest in spirit to the working method of the scientist, although this point is certainly debatable. In this exposition we shall take the liberty of calling our statements of the third kind ' operational definitions ', with the following reservations. Bridgman[1] has demonstrated the difficulty of giving explicit operational definitions for common physical concepts under fairly general circumstances; this is not to be gainsaid. It is a curious feature of electrodynamics, however, that most if not all of the elements of the system, although obeying quite different equations from those of mechanics, can nevertheless be given empirical meaning in terms of the operational definitions of purely mechanical[2] quantities such as force and length. This is in fact the method to be exploited in this paper. We shall presuppose a complete knowledge of mechanics and its associated operational definitions. Our ' operational definitions' (although written for the most part in formal language) will take the form of criteria to be met by, and manipulations to be performed with, *numbers resulting from the experimental measurement of mechanical quantities.* For example equation (3), the definition of numerical measure for charge, requires taking the square root of the product of a certain *force* (an experimentally obtained number) and the square of a certain *distance* (another such number). We shall not, however, concern ourselves with the immense practical difficulties of writing down a complete recipe for, say, force measurement. In this implicit fashion the ' operational definitions ' set up a correspondence between statements of the formal system and the results of experiment. We shall furthermore permit ourselves to use universal quantifiers like ' for all x ' in the ' operational definitions ', with the realisation that the exact procedural meaning of these terms is determined only by the patience of the experimenter.

We may now sketch briefly the types of phenomena to be treated here. When, for example, amber is rubbed with cat's fur, accelerations of the amber and the pelt are produced which cannot be described by statements couched wholly within the language of Newtonian mechanics. The necessity now arises of constructing a new theory to describe new phenomena, and at this point one customarily starts to talk of ' charged bodies ' and ' electrical forces '. Our presupposed knowledge of the theory of mechanics and its associated operational definitions permits us in principle to subtract gravitational, elastic, and inertial forces from the total force observed to act on the cat's fur. Our purpose is to extend the class of forces treated in mechanics to include any force remaining after such a subtraction has been carried out. In the sequel the term ' force ' should be understood to mean this residual value. We may summarise by means of a

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[1] P. W. Bridgman, *The Nature of Thermodynamics*, Cambridge (Mass.), 1943

[2] By ' mechanical ' we mean something normally classed as part of the theory of mechanics. It is remarkable that this relationship between mechanics and electrodynamics can be made to hold in such detail; this fact is perhaps a partial explanation of the extensive effort devoted by Maxwell and others to the construction of mechanical models for electrodynamics.

> *Language Convention.* The non-mechanical interaction between the treated amber and the cat's fur is termed an *electrostatic force.* The bodies so interacting are said to possess *electrostatic charge.* Non-mechanical interaction between the charged amber or cat's fur and other bodies is also said to be due to electrostatic force, and the other bodies are also said to possess electrostatic charge.

We further take from mechanics the concept of point mass, and we shall deal solely with charged point masses in Sections 2, 3, and 4. The empirical meaning of the phrase ' point charge ' will be discussed in Section 5 at greater length.

The axiomatic system will then contain in addition to the mechanical concepts ' force ', ' length ', and ' work ', the new primitive sign ' charge ' (denoted by q, q', q'', . . .) and the new primitive relation ' equality of two charges ' (denoted by ' q equals q' '). The common symbol for equality ' $=$ ' will be restricted to an arithmetical equality between two numbers. The relation ' equality of two charges ' is presumed to possess the usual reflexive, symmetric, and transitive properties. Another primitive sign, ' isolated region ', will be introduced to deal with certain problems occurring in charge measurement experiments, and will specify the conditions under which several of the axioms are to hold.

2 *Charge Measurement*

We introduce the primitive sign ' charge ' by means of

> *Axiom 1.* There exists at least one electrostatic charge.

The next axiom of the system is designed to guarantee the possibility of maintaining a fixed test charge for experiments to be done at our discretion.

> *Axiom 2.* The electrostatic charge on a body kept *in vacuo* remains constant in time, and is independent of the position of the body referred to an arbitrary static coordinate system.

We may therefore choose a unique test charge, denoted by Q, to effect the experiments prescribed in the 'operational definitions' to follow. Our first task is to give such a definition for the statement 'q equals q''. We are immediately faced with the problems posed by induction phenomena (the distortion of pre-existing charge distributions by the introduction of new charges). We must make sure that the test charge used to probe the interaction of a charge distribution does not modify this distribution so strongly that the measurement loses meaning. The existence of these difficulties is customarily acknowledged by the insertion of such phrases as 'a test charge supposed not to disturb the charge distribution' or 'an infinitesimal test charge' into all definitions involving a test charge. Such phrases can be given very little meaning at this stage in the analysis, since we as yet possess no numerical measure for charge and no criterion for what is or is not infinitesimal. It is preferable therefore to treat induction phenomena by means of an axiom.

> *Axiom 3.* There exists at least one non-vanishing volume element which is an isolated region.

If we are able to empirically define the new primitive sign 'isolated region' as a volume in which induction effects are negligible, we shall then be able to do unambiguous experiments with non-vanishing test charges in such isolated regions. Moreover we will not be required to perform limiting procedures in advance of a definition of numerical measure for charge.

It is convenient to give operational definitions (denoted OD) for 'isolated region' and 'equality of two charges' simultaneously. To make the steps more explicit we introduce first the empirical notion of 'equivalence'.

> *OD 1a.* Two charges q and q' are said to be equivalent if and only if for a fixed $\boldsymbol{r}(\mathrm{Q})$ and for every $\boldsymbol{r}(q) = \boldsymbol{r}(q')$,
> $$F(\mathrm{Q}, q) = F(\mathrm{Q}, q'). \tag{1}$$

Here the notation $\boldsymbol{r}(x)$ denotes the radius vector from the origin of a co-ordinate system (in which all charges are at rest) to the charge x, and $F(x, y)$ denotes the force acting on charge x in the presence of charge y. The reader will recall that in this and the following OD's, distances and forces are presumed to be well-defined operationally so that unique measurements of these quantities are possible ; a statement involving the sign '=' like '$\boldsymbol{r}(q) = \boldsymbol{r}(q')$ ' is therefore a statement about the numerical equality of the results of two measurements.

> *OD 1b.* A volume V is said to be an ' isolated region with respect to q and q' ' if and only if for all $\boldsymbol{r}(q)$ and $\boldsymbol{r}(q')$ in V, $\boldsymbol{F}(q,q')$ is an invariant function of $|\, \boldsymbol{r}(q)\, \boldsymbol{r}(q')\,|$.
>
> *OD 1c.* Two charges q and q' satisfy the relation ' q equals q' ' if and only if they are equivalent in a region isolated with respect to q and Q and also with respect to q' and Q.

The system of OD's 1a, 1b, and 1c defines equality through a process of comparison with a standard; if q and q' separately interact with the standard Q in the same way, they are judged to be equal. The convenience of making charge measurements by means of a standard interaction arises of course from the rôle played by charge in the theory, that of coupling constant or interaction strength parameter.

To convince the reader that the system OD 1 is not unnecessarily complicated we must explore more fully the relation between Axiom 3, with its new primitive sign ' isolated region ', and the induction phenomena. The isolated region is plainly a volume element so far removed from free charge distributions, or so symmetrically located with respect to them, that the introduction of charges q and Q into this region does not alter any such distributions. Under these circumstances the interaction between two static charges depends only upon the scalar distance separating them, and is invariant under transformations between static co-ordinate systems. When translated into empirical terms by means of OD 1b, Axiom 3 can be shown to hold in the laboratory. The necessity of introducing the qualification ' with respect to q and q' ' in OD 1b is shown by the following example. Suppose that a charge q is fixed in space, and imagine also a distribution of charged bodies $\rho(\boldsymbol{r})$ constrained by a mechanical potential U to lie at rest in fixed positions nearby. If we now investigate the interaction of q with another charge q', we may find that there exist certain surfaces (in some sense symmetrical to $\rho(\boldsymbol{r})$) along which the condition in OD 1b are fulfilled. The charge distribution $\rho(\boldsymbol{r})$ does not betray its presence, and as far as this experiment is concerned we have established the existence of an isolated region. Suppose now that we replace q' by a charge q'' so large that $\boldsymbol{F}[q'', \rho(\boldsymbol{r})] \geq$ grad U. In this case we shall have overcome the constraints on $\rho(\boldsymbol{r})$ and this distribution will be free to move and become polarised under the influence of q''. Then our region will no longer be isolated in the sense of OD 1b. It is therefore necessary to specify for what range of interaction strengths a region of space is essentially isolated.

In connection with the system 1a, 1b, and 1c, several points deserve further mention. First, the notion of equivalence has been introduced only to clarify the steps in the definitions, and possesses no empirical utility. We may dispense with it entirely by substituting OD 1a into OD 1c. Second, it should be reiterated that the OD's 1a, 1b, and 1c contain all the empirical meaning in the statement 'q equals q''. After a method for measuring charge has been defined, one may show that these definitions imply the equality of the numerical measures of q and q', but it must be emphasised that this result is a theorem of the axiomatic system. Third, such definitions as OD 1, in which a statement is given meaning before its separate elements have been defined, occur frequently in physics and mathematics.[1] It may be necessary for the student to convince himself that the technique is in fact consistent, since this is not always immediately apparent.

We have now the requisite logical apparatus for the interpretation of a number of experiments. For example, now that we have a definite meaning for the phrase 'q equals q'', we may demonstrate empirically that

$$\boldsymbol{F}(q, q') = kr^{-3}\boldsymbol{r} \text{ for } q \text{ equals } q' \tag{2}$$

in a region isolated with respect to q and q'. In this equation

$$\boldsymbol{r} = \boldsymbol{r}(q) - \boldsymbol{r}(q').$$

The constant of proportionality k depends uniquely on the particular pair of equal charges q and q' used in the experiment. This relation is of fundamental importance. Indeed, equation (2) is the central datum for electrostatic charge measurement, since at this point in the analysis a choice of the functional dependence of k on q constitutes an empirical definition of numerical measure for q. Furthermore, the choice is arbitrary within wide limits. We shall make the standard definition by choosing $k(q) = q^2$ and writing

OD 2. The absolute numerical magnitude of a charge q is given by

$$q \equiv |k|^{\frac{1}{2}} = [\mathrm{F}(q,q)r^2]^{\frac{1}{2}} \tag{3}$$

where $\mathrm{F}(q,q)$ is the magnitude of the force between two equal charges

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[1] Some examples are found in Mach's definition of mass, Russell's definition of cardinal numbers, and Cantor's theory of transfinite numbers.

> q, and r is their separation; the measurement is to be done in a region isolated with respect to the two charges.[1]

With a numerical measure for charge in our possession, we can perform as many experiments as we desire to corroborate.

Axiom 4 (*Coulomb's Law*). In a region isolated with respect to q and q',

$$\boldsymbol{F}(q,q') = qq'r^{-3}\boldsymbol{r} \text{ for all } q,\, q'. \tag{4}$$

It is evident that the form of charge dependence in Coulomb's Law is dictated by our choice of the function $k(q)$, since for the case $q = q'$ equations (2) and (4) must be identical. If for example we were to assume $|k(q)| = e^{2q}$, Coulomb's Law might take the rather unwieldy form $\boldsymbol{F}(q,q') = P(q,q')r^{-3}\boldsymbol{r} \exp (q + q')$, where $\mathrm{P}(q,q')$ is a parity factor expressing attractive or repulsive interaction. Conversely if we take Axiom 4 in the form given above, we are led uniquely to OD 2 as the numerical measure for charge. The essential point is this : we may conclude from experiment only that the interaction is bilinear in some function of the charge, and the choice of this function is left to our discretion.[2] In this sense Coulomb's Law is sometimes loosely spoken of as a 'definition of charge'.

The utility of OD 2 rests on the ability of the experimenter to discover or manufacture two equal charges. This possibility is certainly empirically established; it is equally certain that one cannot prove within the axiomatic framework that there are any equal charges in nature. Axiom 5, the Superposition Principle, relates the scalar addition of charge elements to the size of their interactions, and thus furnishes a recipe for the regulation of the charge on a particular body. The reader will notice that Axiom 5 has been phrased in terms of infinitesimal increments of charge, and may question the legitimacy of introducing such an expression. It should be clear by this time that the calculus obeyed by the sign 'charge' in the axiomatic system is that of arithmetic—in other words 'charge' is a number. The phrase

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[1] This definition neglects the sign convention with respect to positive and negative charge; this may be supplied by choosing the sign of the standard test charge Q to be plus, for example, and assigning a plus or minus sign to q according to whether its interaction with Q is repulsive or attractive. There is also implicit in OD 2 an arbitrary choice of units.

[2] The same considerations hold for the gravitational mass appearing in the Newtonian gravity formula. In this case, however, the principle of equality of gravitational and inertial mass fixes the functional dependence on mass as a bilinear one.

' infinitesimal charge ' has therefore the same meaning that ' infinitesimal number ' would have in the integrodifferential calculus. The stipulation that the sign ' charge ' obeys the calculus of numbers is in fact one of the logical and mathematical rules referred to earlier.

In Section 4 the electrostatic field and electrostatic potential will be defined axiomatically in the forms of limits or integrals, well-defined operations over the field of numbers, and it will be argued that this procedure is quite essential to satisfactory definition of field and potential. What is perhaps more disturbing is that the OD's for field and potential will also be given in the form of ' limits '. In the following section we shall therefore try to describe an empirical process which can be called ' taking the limit ' of a set of experimental numbers.

3 *' Empirical Functions ' and their ' Limits '*

Suppose that we make a series of experiments, identical except for the monotonic variation of one of the system parameters affecting the result of the measurement. The results of these measurements can be considered an ' empirical function ' of the variable parameter, and this function can be plotted on a graph as a series of points connected by straight line segments. The curve so obtained frequently suggests the graph of some simple mathematical function. For example, if we charge a capacitor and measure the voltage across it as a function of time during its discharge, we would probably not hesitate to call the curve of voltage versus time an exponential. We would equally readily say that the voltage ' approaches zero as a limit ' as the time increases. What are some of the criteria used in making this assertion ?

It appears that two of the requirements in the mathematical definition of limit cannot be strictly satisfied in the analogous domain of the ' empirical function '; these are the requirements of *smooth* approach and *arbitrarily close* approach to the limiting value of the function. Smooth approach to the limit implies knowledge of the function at *all* points in some neighbourhood near the limit, but since we can measure only at separated points such complete information on the behaviour of the empirical function must necessarily be lacking. We must decide subjectively whether or not the points on our curve are closely enough spaced to justify drawing a smooth curve through them, or even connecting neighbouring points by a straight line. One ultimate boundary on this grid spacing is the accuracy with which the argument of the empirical function can be measured; there is little significance in choosing a grid spacing of 0·01 seconds if the statistical accuracy of a time measurement is only 0·1 seconds. We must also ignore the possibility of a non-denumerable infinity of singular points in the empirical function, since there is no guarantee that any of the measurements will coincide with, and thus disclose, any such singular points.

Secondly, we must accept a cut-off on how closely the limit can be approached. There is an unavoidable uncertainty in the measured value of the empirical function, due to both the sensitivity of the measuring instrument and the statistical variations encountered among measurements. For example, if the voltmeter in the capacitor experiment were calibrated in thousandths of a volt, we could assert with equal vigour that the 'limit' of the voltage was not zero but 10^{-4} volts. Since this error does not in practice disappear, we can give the limit of an empirical function only within some specified range of uncertainty. In other words the empirically deduced limit of a series of experimentally-obtained numbers is itself an experimentally-obtained number, subject to uncertainties and requiring the statement of these uncertainties for its full interpretation.

If the limit at 'infinity' of an empirical function is required there is yet another subjective judgment to be made, namely the finite point at which the empirical function is assumed to no longer vary detectably from its asymptotic value. Referring again to the experiment with the capacitor, if the voltmeter needle were to remain stationary for a long time compared to the period of noticeable fluctuation, we might then conclude that as far as this experiment is concerned 'infinite time' had been reached.

The foregoing discussion presumes that the quantities to be measured do not occur in indivisible quanta larger than the sensitivity of the apparatus. If this is not true, and either the empirical function or its argument are observed to vary discontinuously within the accuracy of the experiment, there are two choices open. One may accept the quantum size as the measurement cut-off but ignore it otherwise; this is done in classical electrodynamics and a 'macroscopic' theory is obtained. Alternatively one may choose to modify the theory to encompass quantities which are not permitted the luxury of continuous variation. The Millikan oil drop experiment is an example of a measurement which defines the boundaries of the macroscopic theory, but does not otherwise modify it. On the other hand this experiment does of course illustrate what we call discrete, particulate, or 'microscopic' phenomena.

The foregoing is hardly an exhaustive discussion of the problems of describing empirical data by mathematical functions, but does describe some of the more obvious subjective processes used in making any such association. The fact remains that the technique is indispensable in the exact sciences, and is the most powerful method of constructing from empirical data statements which can be directly compared with statements of the theory. We may therefore expect that if we ask a laboratory worker to make a particular set of measurements and determine what 'limit', if any, they approach at some point, he will in most cases be able to give an answer in the form of a number with an attached specification of accuracy.

4 *Electrostatic Fields and Potentials*

Let us make

> *Axiomatic Definition 1.* The electrostatic field $\boldsymbol{E}$ at a point $\boldsymbol{r}$ as defined as
>
> $$\boldsymbol{E}(r) \equiv \lim_{Q \to 0} \boldsymbol{F}(Q)/Q \tag{5}$$
>
> where $F(Q)$ is the force exerted on a test charge Q located at the point $\boldsymbol{r}$.

An OD analogous to this definition can be phrased in terms of an 'empirical function'.

> *OD 3.* Obtain a series of measured test charges Q_i, ranging in magnitude from the largest convenient value down to the smallest which can be constructed. Place each charge in turn at the point $\boldsymbol{r}$, measure the (electrostatic) force $\boldsymbol{F}(Q_i)$, on each charge, and construct an empirical function of Q_i from the series of quotients $\boldsymbol{F}(Q_i)$ Q_i. By some standard graphical or numerical procedure test the convergent behaviour of this function as Q_i is decreased; the value, if any, approached by the empirical function is defined as the electrostatic field at the point $\boldsymbol{r}$.

Note that the use of a limiting procedure frees us from specifying that the experiment be done in an isolated region. In essence, taking the limit Q → 0 makes the entire space an isolated region. If restricted solely to finite isolated regions our definition would fail in the cases of greatest interest (regions near charge distributions), and the field concept would lose its tremendous power as a descriptive scheme. The significance of the field concept in classical physics is that the field can be considered to store and to transport energy and momentum, thus preserving the conservation laws in action-at-a-distance phenomena. The ability of the field to maintain the integrity of conservation laws depends on the field's being continuously defined everywhere, with the exception of possible singularities at the field sources. Furthermore, since a given volume is isolated only with respect to a certain range of interaction strengths, the electrostatic field defined in terms of isolated regions would in general be multivalued. Therefore all restrictions relative to isolated regions must be eliminated in the field definition, and for this purpose the use of a limiting procedure seems unavoidable.

It is not evident *a priori* that the limit in equation (5) exists, and this question is worthy of an explicit answer. Our proof depends on a frequently overlooked linearity axiom of the theory. We now introduce

> *Axiom 5* (*Principle of Differential Superposition*). Let $\{\delta q_i\}$ be a set of infinitesimal charges. Then
>
> $$F(Q, \sum_i \delta q_i) = \sum_i F(Q, \delta q_i) \tag{6}$$
>
> where $F(Q, \sum_i \delta q_i)$ represents the total force on Q due to $\{\delta q_i\}$.

We may now prove

> *Theorem 1* (*Uniqueness of Field*). The limit described in Axiomatic Definition 1 exists almost everywhere.

From Axiom 5, the interaction of the charge distribution to be described by a field $\boldsymbol{E}$ can be represented by the interactions of some set $\{\delta q_i\}$, the choice of which set does not depend on Q. Furthermore, for all Q less than or equal to some Q_0 the region around the point $\boldsymbol{r}$ is isolated. It then follows from equations (5) and (6) that

$$\boldsymbol{E}(\boldsymbol{r}) = \lim_{Q_0 \to 0} \Big[\sum_i \boldsymbol{F}(Q_0, \delta q_i)\Big] / Q_0 \tag{7}$$

and using equation (4) we find

$$\boldsymbol{E}(\boldsymbol{r}) = \sum_i \delta q_i \,|\boldsymbol{r} - \boldsymbol{r}_i|^{-3} (\boldsymbol{r} - \boldsymbol{r}_i) \tag{8}$$

Thus $\boldsymbol{E}(r)$ exists except at the points $\boldsymbol{r} = \boldsymbol{r}_i$, and the desired theorem is proved.

Our final objective is to introduce the concept of electrostatic potential. Let us make

> *Axiomatic Definition 2.* The electrostatic potential at a point r is defined as
>
> $$\phi(\boldsymbol{r}) \equiv -\int_{\infty}^{\boldsymbol{r}} \boldsymbol{E} \,.\, d\boldsymbol{l} \tag{9}$$
>
> where the line integral is along an arbitrary path.

It is easy to show from Axioms 4 and 5 that curl E vanishes, and from this there follows

Theorem 2. $\phi(\boldsymbol{r})$ exists and is single-valued.

For purposes of clarity we give the OD for electrostatic potential in two steps; for brevity we shall use mathematical notation and leave to the reader the elaboration of OD's 4 and 5 in language similar to that of OD 3.

OD 4. The electrostatic potential difference between the points r_1 and r_2 is

$$\mathrm{PD}(\boldsymbol{r}_1, \boldsymbol{r}_2) \equiv \lim_{Q \to 0} \mathrm{W}_{\mathrm{Q}}(\boldsymbol{r}_1, \boldsymbol{r}_2)/\mathrm{Q} \tag{10}$$

where $\mathrm{W}_{\mathrm{Q}}(\boldsymbol{r}_1, \boldsymbol{r}_2)$ is the work done on a charge Q in moving it quasi-statically from $\boldsymbol{r}_1$ to $\boldsymbol{r}_2$ by an arbitrary path.

OD 5. The electrostatic potential at a point $\boldsymbol{r}$ is

$$\phi(\boldsymbol{r}) \equiv \lim_{\boldsymbol{r}' \to \infty} \mathrm{PD}(\boldsymbol{r}', \boldsymbol{r}). \tag{11}$$

Note again that the use here of the limiting procedure is essential. OD 4 may be given without a limiting procedure by specifying that the experiment be done in an isolated region. Then, however, OD 5 will fail, since we cannot be sure that the arbitrary path of OD 4 will always pass through isolated regions as $\boldsymbol{r}' \to \infty$.

5 *Point Charges and Coulomb's Law*

Our discussion so far has been restricted to interactions among point charges, and Coulomb's Law has been represented as exact only when applied to point charges. In a sense our axiomatic system is an implicit definition of the notion 'point charge'. It is therefore of interest to investigate what empirical meaning this concept may have, apart from being a point mass (a well-defined term in mechanics) bearing an electrical charge. Intuitively we understand by the phrase 'point charge' a charge distribution with vanishing spatial extension; we may then expect that the exact Coulomb's Law is in some way a limit of the interaction laws for extended charge distributions.

Consider a sphere, bearing a charge q, in an isolated region whose dimensions are large compared to the diameter of the sphere. We shall probe the interactions of q with a test charge Q which we assume to have small dimensions compared to the sphere (this restriction is convenient but not essential). Since the region is isolated we know that Q does not noticeably perturb the charge distributions external to the region, but we are not assured that Q leaves the distribution of q

undisturbed. If the sphere is a conductor, the distribution will in fact polarise until the surface of the sphere is an equipotential. Under such circumstances Coulomb's Law does not hold for the interaction between q and Q. Now the change distribution on the sphere may be expanded in a series of multipoles where the potential of the nth multipole depends as $r^{-(n+1)}$ on the separation r between sphere and test charge. As r is made larger, the effects of the higher order terms in the expansion decrease until at last only the monopole term contributes to the interaction. In this limit the interaction measures only the product of the test charge Q and the sphere's total charge q, and Coulomb's Law holds with arbitrary precision. If we relax the restriction on Q and allow it to be distributed over a body comparable in size to the sphere, it too will become polarised. However, the distribution of Q can also be given as a multipole expansion and the argument carries through as before.

This limiting process rigorously eliminates all shape-dependent effects on the source of the field, and permits isolation of the 'true' electrostatic interaction. At the same time it can be taken as an empirical definition of the term 'point charge interaction'. The question of infinite point-charge self-energies is also to some extent avoided, since the procedure does not permit arbitrarily close approach to the field singularity of the point charge.

We see therefore that the interactions between charge distributions of finite extension obey Coulomb's Law as a limit. Note that this procedure does not apply to infinitely extended charge distributions; one cannot for example define operationally the total charge on an infinite conducting plane. This fact corresponds to the analytical statement that in general the monopole moment of an unbounded charge distribution does not exist.

It may be of interest to make one further general observation. In the case of electrostatics, as in other portions of classical theory, most of the fundamental axioms are amenable to direct experimental verification ; we may identify statements of fact corresponding to the axioms by means of the OD's, and examine these pairs for consistency. In the more recent theories of modern physics empirical statements corresponding to the axioms are frequently inaccessible to direct experiment, and even meaningless in some cases. In the special theory of relativity the axiom 'The velocity of light is the same in all inertial frames' cannot be directly tested in many reference frames of great interest (such as the rest frame of a relativistic unstable particle) because of the great experimental difficulties. In quantum theory the situation is even more remote. Here the elements of the axioms (operators and wave functions) have *by definition* no OD's and therefore no empirical counterparts susceptible to measurement; the theory says that they are not ' observables '. One cannot speak of an experiment corroborating Schrödinger's equation in the same sense as an experiment corroborating Coulomb's Law; one can only do an experiment corroborating some theorem deduced from Schrödinger's equation.[1] In these cases the physicist must be content to glean empirical support for his theories from verification of certain theorems rather than axioms. This of course does not detract from either the utility or significance of these theories, but may perhaps be a standard by which one theory is judged more ' abstract ' than another.

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[1] Of course due to the tautological character of the theorem it can be viewed merely as a restatement of its premises with nothing added to their content. The statements of premises are however more remote from experience (in quantum theory) than many of the theorems, and there is always the possibility of error (or more commonly, inadequate approximations) in the derivations.

NOTE AND COMMENT

'Lying' and the Compleat Robot

Michael Scriven has tried, with great subtlety, to indicate what the complete robot will be.[1] He has even suggested how far one can go in eliciting the answer from the robot himself (or herself)[2] if it is built in a certain way. But I think the question whether a robot can lie requires some further discussion than the extremely casual one given by him in his study.

'Lying' involves telling something different from what one knows to be the case. This, of course, is only a first approximation. Lying is by no means confined to verbal reporting only. One may lie in a hundred ways. The gesture, the expression, the turn of an eye, the inflexion of a voice all may be used for lying even more effectively than the utterance of a sentence. Rather, a sentence which ostensibly is false may be uttered in such a way as to convey just the opposite. Or, equally, a true sentence may be embedded in a context of expressive gesture which turns it into an effective lie. 'Lying', then, is a rather complex affair and needs exploration both on the intentional and the performatory side before the Compleat Robot may do what he is expected to do.

One need not exactly *know* the case, in order that the lie may be a lie. One's belief for that purpose is sufficient. If I believe that such is the case and try to produce, by whatever means, a belief contrary to that which I believe, this will be regarded as 'lying' even if my own belief is *actually* mistaken. In such an event, it will be difficult to suspect that what I told was a lie, for it will be found to be true and normally taken as having been uttered in that way. It is not, thus, the *actual* contrariety of what is uttered or expressed to whatever happens to be the case that makes a lie a 'lie'. Rather, it is the contrariety to what is *believed* to be the case that turned it into a 'lie'. And 'believing' is a very, very 'intentional' affair. There is, of course, a performatory aspect to 'belief' and we do try to square a man's professed beliefs with his behaviour. We even go so far as to infer unconscious beliefs that seem to be involved in the behavioural choices of persons. But, firstly, one does not generally speak of the beliefs of animals, not to speak of the beliefs of robots. There seems something incongruous in saying that the behaviour of the animal implies that he believes in such and such beliefs. For example, the statement 'my dog does not seem to believe in an after-life' makes no sense as its opposite doesn't do either. This may suggest that the ascription of belief to some object can make sense only if it can be said, in some sense, to disbelieve also. Secondly, there seem to be beliefs whose performatory aspect is too much confined to their verbalised expression. For example, if someone says that he believes in ghosts or that there was a life before birth or that there will be life after death, it is not quite easy to see what further performatory behaviour on his part is *necessarily* required for our believing in his belief. The person believing in ghosts may try table-tipping or show fear in the dark but he may, equally well, not do so. Yet, the mere verbalised expression on his part is, in most cases, sufficient to induce us to believe in the truth of his statement. On the other hand, no one will believe in this belief if a machine was made to utter such a verbalised expression.

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[1] *Dimensions of Mind:* A symposium, ed. by Sidney Hook, New York, 1960.

[2] The question of sex in robots has, as far as I can see, not been discussed up till now.

The difficulties with regard to 'lying' may increase still further if we take into account the degrees of belief which so usually characterise our psychical life in this respect. I wonder if it will be called a lie if I try to produce a greater sense of conviction in the other person than I myself possess in the matter. This introduces another aspect which may be regarded as instrinsic to lying, that is, it has a purpose or an end to achieve without which it will not be communicated at all. I may be mistaken about the way I adopted to make my lie succeed in its function, but that I do want it to succeed is the very *raison d'être* of lying. A pure lying for its own sake will not be lying. It will be just joking or having fun or imagining or telling a tale or anything else. The point of lying is that the other person should not be able to guess that it is a lie and the success of lying can only be determined if we know what the lie was for.

These considerations, I think, are extremely important for even the performatory aspect of lying to which Michael Scriven has confined himself in his paper. The point is how can one determine the performatory aspect unless, to some extent, one has determined what 'lying' is? Scriven, I am afraid, has not even raised the issue in his paper. He writes: 'We also introduce the robot to the concept of truth and falsity and explain that to lie is to utter a falsehood when the truth is known. . . .'[1] This obviously is an extremely loose kind of statement. One does not introduce robots to 'concepts' nor does one 'explain' anything to them. One just builds into them structures which perform the behavioural responses which we consider to be relevant to the doing of a certain action or being a certain sort of activity. The proper question would then have been to ask what is the performatory activity which we would have to build in a machine so that it may be said to 'lie' when it performs that sort of behaviour? Michael Scriven has not even raised this question, though it is so central to his whole argument. Perhaps he thought the matter quite simple, not worth a detailed discussion at all.

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[1] Michael Scriven, op. cit. p. 141

However, if we do think it out, there seem to be difficulties which do not seem quite so easy to overcome. This may, of course, be overcome or circumvented but it has to be shown how. For example, following Mr Scriven's explanation to the robot, we may build in a structure which would make the robot always say the opposite of what it ' knows '. Supposing we do it, would this be ' lying ' as we know it in a human being. Surely not, for here the lying would become so mechanical that the person noting the robot's responses would quickly find that the robot is functioning differently and will correct his interpretations and behave accordingly. The more important point, however, is that this will not change the robot's behaviour at all. It will continue to act or speak the lie which has been built into him even when it (the lie) has ceased to perform its function. We may, of course, try to build a robot which will change its behaviour on noticing that its lie does not work. But what will this exactly mean? How will the robot notice, say, my disbelief in his reporting. I need not do anything at all to disbelieve what someone is saying. Also, the range of actions that I may do as a result of my disbelief are too large and varied to be built into the robot's system as stimuli arousing it to change his performatory lying. Not only this, he is to be built in such a way that on getting the stimuli which are the performatory equivalent of disbelieving, he should be able to devise new ways of deceiving the disbelievers once again.

On the other hand, instead of making the lie mechanical we may just ' randomise ' it in the robot's system. Now the robot will not lie every time it knows what is the truth, but only on chance occasions. This will hardly be ' lying ' at all, for the robot may come to lie at a moment when it interferes with its own interests. In other words, if the robot were conscious in such a situation he will not lie at all. One may, of course, try to exclude such situations by building into the robot mechanisms which will exclude such self-defeating lying on his part. But, then, the rest of the lying would be some sort of a joke or a piece of fictional imagination if it is not also geared to some ends which the robot has been built to achieve.

This, perhaps, is the central point. Can ' lying ' be adequately understood even in its performatory aspect without reference to the purpose it seeks to achieve? Michael Scriven has gone on to write: 'We then add a circuit to the robot, at a special ceremony at which we also christen it, which renders lying *impossible* regardless of conflict with other goals it has been told are important.'[1] Professor Scriven has forgotten that if the poor robot cannot but tell the truth, it is a robot indeed. ' Truth-telling ', like ' lying ', cannot be mechanised, that is, made compulsory without losing all that we connote by these terms in their human context. Further, it is not so easy to tell the truth as Scriven seems to suppose. Even the question what is truth has been the subject of unending debate among those who have tried to think about the matter.

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[1] Michael Scriven, op. cit. p. 141 italics mine

Between the mechanical and the random may lie the area where we may try to build in 'lying' in the service of some purpose which the robot can vary if it finds that it does not succeed in the achievement of that purpose. I wonder what will be the engineering problems set by such a task. But, at least this much seems fairly clear to me that if the robot aspires to be human, it cannot be made impossible for it to tell a lie, though what it will mean for a robot to tell a lie is not clear either. In any case, there seems little doubt that a lot of analysis is needed even on the performatory level to say that a robot is 'lying' or 'telling the truth'. And if Scriven's experiment depends on the impossibility of the robot's telling anything but the truth, his experiment is bound to fail if simultaneously he is trying to make it as human as possible, that is, if it is being made in principle 'unpredictable in a way essentially similar to the way human beings are.'[1] If, on the other hand, the robot's unpredictability is given up in the matter of 'truth-telling' then whatever the robot's answer to Scriven's question, we may rest assured that it is not human in the sense in which we consider ourselves to be human, that is, free to tell the lie or truth as it pleases us.[2]

DAYA KRISHNA

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[1] Michael Scriven, op. cit. p. 122

[2] It is interesting to note the similarity of the robot-maker's dilemma to that of God with respect to man. If man is given freedom, then he may develop all sorts of undesirable qualities and if he is not given freedom, there is no virtue in his love or prayer or truth-telling or anything else that he does.

DISCUSSIONS

More About Lorentz Transformation Equations

Recently G. H. Keswani has published a paper under this title (this *Journal*, 1960, **11**, 50) in which he tries to establish a contradiction between the principle of Relativity and the Lorentz transformation equations, and infers from this the existence of a preferential system of reference (ether). I wish to point out that these conclusions derive from an elementary error.

He considers (p. 52) two co-ordinate systems $S(x, y, z, t)$ and $S'(x', y', z', t')$ with parallel axes, S' moving in the x-direction relative to S with the velocity v. Then the connecting Lorentz transformation is

$$x' = \frac{x - vt}{\sqrt{1 - v^2/c^2}},\ y' = y,\ z' = z,\ t' = \frac{t - (v/c^2)\,x}{\sqrt{1 - v^2/c^2}} \tag{1}$$

Then he considers a spherical light wave spreading from the common origin O at the time $t = t' = 0$ and says (I quote): 'In S, the intercepts of the three axes are

$$x = r,\ y = r,\ z = r \text{ at time } t = r/c,\text{'} \tag{2}$$

He inserts these values in the Lorentz transformation as if they were meant to be the co-ordinates of one point (not of three), and calculates the co-ordinates of this point in the system S'. Thus he finds

$$x' = r\left(\frac{c - v}{c + v}\right)^{\frac{1}{2}},\ y' = r,\ z' = r,\ t' = \frac{r}{c}\left(\frac{c - v}{c + v}\right)^{\frac{1}{2}}. \tag{3}$$

He concludes that the 'three points' in S' are not on a sphere but on an ellipsoid. What his calculation actually means is the transformation of a single point from S to S', namely the corner opposite to O of the cube with sides parallel to the axes in S and with the length r, at the time when a light signal from the origin reaches it. There is, of course, not the slightest contradiction against relativity in his results.

To follow his programme, one would have to consider the three axial points at distance r at the same time $t = r/c$, which in S are given by:

$$\left\{\begin{array}{l} P_1\colon\ x_1 = r,\ y_1 = 0,\ z_1 = 0,\ t_1 = r/c, \\ P_2\colon\ x_2 = 0,\ y_2 = r,\ z_2 = 0,\ t_2 = r/c, \\ P_3\colon\ x_3 = 0,\ y_3 = 0,\ z_3 = r,\ t_3 = r/c. \end{array}\right. \tag{4}$$

These all lie on the spherical wave front

$$x^2 + y^2 + z^2 = r^2,\ \text{with } r = ct. \tag{5}$$

The Lorentz transformation leads to

$$\left\{\begin{array}{l} P_1\colon\ x'_1 = r\left(\dfrac{c - v}{c + v}\right)^{\frac{1}{2}},\quad y'_1 = 0, z'_1 = 0,\ t'_1 = \dfrac{r}{c}\left(\dfrac{c - v}{c + v}\right)^{\frac{1}{2}}, \\ P_2\colon\ x'_2 = -r\dfrac{v}{\sqrt{c^2 - v^2}},\ y'_2 = r,\ z'_2 = 0,\ t'_2 = \dfrac{r}{c}\dfrac{c}{\sqrt{c^2 - v^2}}, \\ P_3\colon\ x'_3 = -r\dfrac{v}{\sqrt{c^2 - v^2}},\ y'_3 = 0,\ z'_3 = r,\ t'_3 = \dfrac{r}{c}\dfrac{c}{\sqrt{c^2 - v^2}}. \end{array}\right. \tag{6}$$

These three events are not simultaneous in S′; the first P_1 is on the wave-front

$$x'^2 + y'^2 + z'^2 = r'^2, \text{ where } r' = r\left(\frac{c - v}{c + v}\right)^{\frac{1}{2}} = ct', \tag{7}$$

the other two, P_2 and P_3, on the different wave front

$$x'^2 + y'^2 + z'^2 = \bar{r}'^2, \text{ where } \bar{r}' = r\frac{v}{\sqrt{c^2 - v^2}} = ct'. \tag{8}$$

It is obvious that the radii (and the corresponding transmission times) are not the same for the 'longitudinal' point P_1 and the two transverse points P_2 and P_3. But this does not, of course, mean that the wave front in S′ is ellipsoidal; only its time of expansion is different for the three points when observed in S′.

By drawing a Minkowski diagram in three dimensions x,y,t (omitting z) one can see all this intuitively.

The difficulties met by Mr Keswani in the connection between the principle of relativity and the Lorentz transformations are due only to primitive errors in his calculations. I regret that such faulty considerations are put before the readers of a philosophical periodical as they thus are made to believe that physicists do not know what they are talking about.

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Reply to Professor Born

The question is: how is a spherical wave-front of light in S observed in S'?

We said that it is a sphere if we consider the principle of relativity, but that it is an ellipsoid if we apply the Lorentz equations for transformation. Born says that the wave-front in S' is not ellipsoidal.

What did the prophet himself say? In his original paper of 1905 Einstein said

> The wave under consideration is therefore no less a spherical wave with velocity of propagation c when viewed in the moving system . . . (*The Principle of Relativity*, Electrodynamics of Moving Bodies, 1923, p. 46)

But a little later, when dealing with the transformation of energy of light-rays he stated

> The spherical surface—viewed in the moving system—is an ellipsoidal surface . . . (op. cit. p. 57)

These two statements were quoted in our paper but Born does not comment on them.

Einstein obtained an ellipsoid as follows (op. cit. p. 57). Here we use our notation. The equation of the spherical surface expanding with the velocity of light in the 'stationary' system (i.e. S) is:

$$(x - lct)^2 + (y - mct)^2 + (z - nct)^2 = r^2 \tag{1}$$

where l, m, and n are the direction-cosines of the wave-normals. For $t = o$, we get the space-equation of the wave-front, $x^2 + y^2 + z^2 = r^2$ in S. Putting

$$x = (x' + vt')/\sqrt{1 - v^2/c^2},$$

$t = (t' + vx'/c^2)/\sqrt{1 - v^2/c^2}$, $y = y'$, $z = z'$ and $\beta = 1/\sqrt{1 - v^2/c^2}$, equation (1) is transformed into:

$$(\beta x' + \beta vt' - \beta lct' - \beta lx'v/c)^2 + (y' - \beta mct' - \beta mx'v/c)^2 + (z' - \beta nct' - \beta nx'v/c)^2 = r^2.$$

Setting $t' = o$, Einstein obtained the following spatial equation of the ellipsoidal surface in the moving system:

$$(\beta x' - 1\beta x'v/c)^2 + (y' - m\beta x'v/c)^2 + (z' - n\beta x'v/c)^2 = r^2 \qquad (2)$$

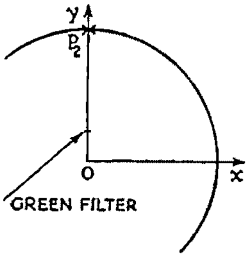
Gone is the invariant sphere of our assumptions!

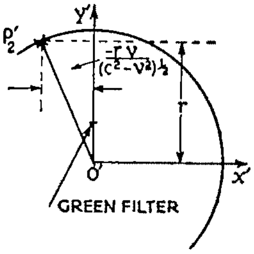
As a matter of fact the direction-cosines are not invariants, but writing l', m', and n' for l, m, and n in equation (2) makes no difference. However, we must state that we are only quoting and trying to explain Einstein.

Born transforms the 4-dimensional points $P_1 : [r, o, o; r/c]$, $P_2 : [o, r, o; r/c]$ and $P_3 : [o, o, r; /cr]$ into $P'_1 : [r\sqrt{(c - v)/(c + v)}, o, o; r/c \cdot \sqrt{(c - v)/(c - v)}]$,
$P'_2 : [- rv/\sqrt{(c^2 - v^2)}, r, o; r/c \cdot c/\sqrt{(c^2 - v^2)}]$
and $P'_3 : [-rv/\sqrt{(c^2 - v^2)}, o, r; r/c \cdot c/\sqrt{(c^2 - v^2)}]$. Events P'_1 and P'_2(or P'_3) are not simultaneous. And here Born comes to a stop, having proved nothing. He was to have shown that the wave-front is spherical in S'. In fact we can proceed further in the same vein. At time $t' = r/c \cdot c/\sqrt{(c^2 - v^2)}$ the wave-front on the x' axis is at $rc/\sqrt{c^2 - v^2}$. The events $[rc/\sqrt{(c^2 - v^2)}, o, o; r/c \cdot c\sqrt{(c^2 - v^2)}]$,

$$[- rv/\sqrt{(c^2 - v^2)}, r, o; r/c \cdot c/\sqrt{(c^2 - v^2)}]$$

and $[- rv/\sqrt{(c^2 - v^2)}, o, r; r/c \cdot c/\sqrt{(c^2 - v^2)}]$ are then simultaneous in S' and the wave-front thus discovered clearly lies on a sphere of radius $\sqrt{[r^2 + r^2v^2/c^2 - v^2]} = rc/\sqrt{c^2 - v^2}$ at time $r/c \cdot c/\sqrt{(c^2 - v^2)}$. What more could be desired? But let us pause and reflect. Have we transformed points lying on a sphere of S? Not at all. The point P_1 of S is *not* transformed but another point $P_{11} : [rc/(c - v), o, o; r/(c - v)]$ of S. We have obviously transformed three points non-simultaneous in S and lying on an ellipsoid, now in S, into three points simultaneous in S' and lying, as a poor consolation, on a sphere in S'. The situation is somewhat like that of a clown in a circus, who drops as many packets as he picks up and when picking up again, drops the ones he picked up earlier.





There are contradictions galore. Let us look at P_2 and P'_2. Imagine a tiny, say, green filter placed across the y and y' axes at a distance from the origins $<< ct$. The space-point (o, r, o) on the y axis in S receives green light (emitted white) but the transformed space-point in S', $(-rv/\sqrt{(c^2-v^2)}, r, 0)$, which does *not* lie on the y' axis, is green by transformation but white according to the principle of relativity. The two systems are simply not equivalent. Even the abracadabra of non-simultaneity cannot manage to dispel this contradiction. The 4-dimensional points on the surface $x^2+y^2+z^2-c^2t^2=0$ are undoubtedly transformed by Lorentz equations into *some* points on the surface $x'^2+y'^2+z'^2-c^2t'^2=0$ by the simple device that $x'^2=x^2-\phi$ and $c^2t'^2=c^2t^2-\phi$ where $\phi=(x^2v^2/c^2-2xvt+v^2t^2)/(1-v^2/c^2)$, but when we depart from these algebraic considerations and go into the physical consequences of these transformations, antinomies appear.

Following the principle of relativity, *we* took the equations of the expanding wave as $x^2+y^2+z^2=c^2t^2$ (sphere with origin of light-pulse at O) and $x'^2+y'^2+z'^2=c^2t'^2$ (sphere with origin of light-pulse at O'). From these equations we derived the Lorentz equations as usual. We then returned to apply these equations to the very phenomenon on which they were based. The spatial co-ordinates of the points of intersection of the wave-front with the three axes of the two systems are:

In $S(x, 0, 0)$, $(0, y, 0)$ and $(0, 0, z)$ at time t.
In $S'(x', 0, 0)$, $(0, y', 0)$ and $(0, 0, z')$ at time t'.

Mark the second equation. Faithful still (until a contradiction appears) to the principle of relativity, we take the centre of the wave-front in S' at O' and the equation of the wave-front in S', $x'^2+y'^2+z'^2=c^2t'^2$, as we first assumed it. For the instant t we put $x=y=z=r=ct$ in S and the distances x', y' and z' in S' are then found by transformation to be $r\sqrt{(c-v)/(c+v)}$, r and r at time $t'=r/c\sqrt{(c-v)/(c+v)}$ There is no cube to be seen here as Born avers.

We submit that, when there is something wrong, it is usually with our 'primitive' notions to which we may profitably return again and again. We are thankful to Professor Max Born for the stimulus he has provided with his discussion.

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G. H. Keswani

Professor Dingle on Falsifiability: A Second Rejoinder

In Professor Dingle's Reply[1] to my earlier Rejoinder to him,[2] he writes: 'Professor Grünbaum's statement that the Kennedy-Thorndike experiment could falsify the contraction hypothesis is simply incorrect. The experiment gave a null result, which was consistent with the contraction and the "time dilatation" operating together. . . . No conceivable result of the experiment could have falsified the contraction hypothesis, or even increased the probability of its falsity.' And concerning my claim that there is experimental evidence supporting Einstein's postulate that the velocity of light is independent of the velocity of its source against Ritz's rival hypothesis, Professor Dingle says: 'Professor Grünbaum has apparently not seen a paper in *Mon. Not. R.A.S.*, 1959, **119**, 67. There is at present no experimental evidence at all either for or against this postulate.'

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[1] H. Dingle, this *Journal*, 1960, 11, 145

[2] A. Grünbaum, this *Journal*, 1960, 11, 143-145

My reasons for now offering a concluding rejoinder to Professor Dingle are the following: (1) I deem it essential to call attention to the error vitiating the conception of falsifiability and confirmability which underlies his rejection of my interpretation of the Kennedy-Thorndike experiment; (2) unlike Professor Dingle, I do attach importance to a correct assessment of the *ad hoc* charge against the Lorentz-Fitzgerald contraction hypothesis. For such logical clarity not only serves to articulate the general logical features of an *ad hoc* hypothesis but also throws light on a significant episode in the pre-history of the Special Theory of Relativity; and (3) Professor Dingle neglected to mention that he himself is the author of the paper cited by him as having invalidated the generally held view, which I had endorsed, that there exists observational evidence cogently supporting Einstein's postulate against that of Ritz.

In order to make quite clear what is at issue between Professor Dingle and myself, I shall show first that attention to an important conditional clause in my original Note could have obviated his erroneous supposition that my interpretation of the Kennedy-Thorndike experiment rests on the illicit neglect of an implicit auxiliary hypothesis concerning the rates of moving clocks. And it will then become apparent that Professor Dingle's criticism turns entirely on dubious principles of inductive reasoning, which he invokes with equanimity as if they were integral to our funded knowledge of scientific methodology.

Professor Dingle overlooked unfortunately that I had stated explicitly that within the framework of the aether theory as modified by the Lorentz-Fitzgerald contraction hypothesis, a *positive* result in the Kennedy-Thorndike experiment is *predicated* on the assumption that 'the period of the light source [as measured by the clocks in the *aether system*] does *not* itself depend upon this velocity [relatively to the aether system]'.[1] But this proviso is tantamount to the recognition that the *positive* outcome of the Kennedy-Thorndike experiment is *predicated* on the original aether-theoretic assumption that the rates of clocks in moving inertial systems are *the same* as those of the clocks in the privileged aether system. To see this, we note first that the *proper* frequency of a moving light source (i.e. the frequency as measured by the clocks of the *moving* system) is *the same* as the frequency which that light source would exhibit when at rest in the aether system. And it is then patent what would be the consequence of assuming a *dependence* of the period of the light source, as measured by the clocks of the *aether system*, on the velocity of that source relatively to the aether by postulating a reduction in the *frequency* of the light in the ratio of $\sqrt{1 - \beta^2}: 1$, as measured by the clocks of the *aether system*. Clearly what follows is the occurrence of the 'time-dilatation' in the sense of the reduction in the rates of moving clocks by a factor of $\sqrt{1 - \beta^2}$ as compared to the clocks of the aether system. As Professor Dingle rightly remarks, the coupling of the assumption of this time dilatation with the Lorentz-Fitzgerald contraction hypothesis does indeed yield a null outcome for the Kennedy-Thorndike experiment: since, as measured by the clocks of the *aether system*, the round-trip times for the vertical and for the *contracted* horizontal arms of that experiment are respectively given by[2]

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[1] A. Grünbaum, this *Journal*, 1959, **10**, 49

[2] Ibid.

$$T_v = \frac{2L}{c\sqrt{1-\beta^2}} \qquad \text{and} \qquad T_h = \frac{2l}{c\sqrt{1-\beta^2}},$$

the corresponding round-trip times registered by the relatively *retarded* clocks of the *moving* system would be *independent* of the velocity of the apparatus with respect to the aether, thereby precluding a shift in the interference fringes due to the changes in that velocity.

The issue between Professor Dingle and me arises therefore *not* from my having been unaware of the fact that the null outcome of the Kennedy-Thorndike experiment was ' consistent with the contraction and the " time dilatation " operating together '. Instead, our disagreement centres on whether this logical fact can be used to show that the null outcome of the Kennedy-Thorndike experiment may *not* be construed as having falsified the Lorentz-Fitzgerald contraction hypothesis, as he claims.

In order to demonstrate the untenability of this thesis of his, we must ask whether the hypothesis that the contraction and the time dilatation operate together is a *methodologically* acceptable interpretation and inductively a legitimate alternative to my construal of the null outcome as having falsified the contraction hypothesis. More specifically, the question before us is which of the following two interpretations of the negative result of the experiment is *methodologically* the more warranted one:

(i) The Lorentz-Fitzgerald contraction hypothesis is *falsified* in the sense of being (highly) disconfirmed, though not *conclusively* falsified, and this *independent* test of it shows that its espousal in response to the Michelson-Morley experiment was not *ad hoc*, and

(ii) The compound auxiliary hypothesis comprising *both* the Lorentz-Fitzgerald contraction *and* the time dilatation is *confirmed.*

It is my contention that interpretation (i) is warranted methodologically while (ii) definitely is *not.* Professor Dingle maintains without supporting argument that interpretation (i) is illicit merely because there exists in the purely logical sense an alternative interpretation (ii) *affirming* the Lorentz-Fitzgerald contraction. But this reasoning would be valid, only if he could show—which he cannot—that the proffered rival (ii) will pass muster *methodologically* by *not* being demonstrably *ad hoc.* As I emphasised, however, in my original Note, *unlike* the version of the aether theory containing *only* the modification that there is a Lorentz-Fitzgerald contraction, ' a version of the aether theory incorporating *both* the Lorentz-Fitzgerald contraction *and* the Lorentz-Larmor-Poincaré time dilatation is vulnerable to the *ad hoc* charge.[1] For the assumption that the Lorentz-Fitzgerald contraction *and* the time dilatation operate together does *not in principle* lend itself to any independent test whatever. And it is, of course, precisely this latter fact which provides the justification for *rejecting* the *doubly*-amended variant of the aether theory despite its *consistency* with observational findings and for accepting Einstein's theory instead.

It is clear now that Professor Dingle's denial of the falsifiability (disconfirmability) of the Lorentz-Fitzgerald contraction hypothesis by the null outcome of the Kennedy-Thorndike experiment founders on his attempt to preserve the hypothesis from refutation by dint of incorporating it in an augmented auxiliary hypothesis which is indeed *ad hoc.* Accordingly, his criticism has failed to refute my original assessment of the

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[1] A. Grünbaum, this *Journal*, 1959, **10**, 50

Kennedy-Thorndike experiment as being capable of furnishing an independent test of the contraction hypothesis.

What can be said of the arguments put forward by Professor Dingle in the paper of 1959 cited by him (and in his earlier paper in *Bull. Inst. Phys.*, 1958, **9**, 314) in his endeavour to show that extant experimental evidence does *not* lend support to Einstein as against Ritz? I shall refrain from the lengthy rebuttal which his altogether unconvincing theses would require, since I am content to rest my case on the following two refutations of the Ritz hypothesis: (i) the critique given in W. Pauli's careful treatment,[1] and (ii) a very recent report of an experiment by A. M. Bonch-Bruevich published in English translation in *Physics Express*, November 1960, pp. 11-12, which explicitly repudiates Dingle's contention.[2] Bonch-Bruevich writes: 'a direct experiment showing that the velocity of light is independent of the velocity of motion of the source of emission relative to the observer is meaningful in view of the fundamental significance of the [Einstein] postulate. Dingle discusses this (*Nature*, 1959, **183**, 1761) in a recently published comment. From his comment it follows that the author is not aware of the results obtained from an experiment on the direct verification of the second [Einstein] postulate which was performed in 1955.[3]

'The experiment consisted of comparing the times t_1 and t_2 required to traverse the path $L = 2000$ m by light emitted from two moving sources which were the equatorial limbs of the sun. . . . The results of the experiment seem to us convincing, and their repetition in another form (for example, when using the radiation from excited atoms or ions as the moving source) will probably not be of great interest at the present time.'

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A REPLY TO PROFESSOR GRÜNBAUM'S REJOINDER

THE point at issue is very simple, namely: could the Kennedy-Thorndike experiment have falsified the contraction hypothesis? Two results were possible: (i) a fringe shift; (ii) no fringe shift. In case (i) the most natural explanation would have been contraction, alone or supplemented by some modification, depending on the amount of shift, of atomic frequency. In case (ii) the most natural explanation would have been contraction, supplemented by the particular 'time dilatation' which Lorentz had suggested in 1904.[4] In neither case, therefore, could the result conceivably have falsified the contraction hypothesis. It does not matter in the least whether any alternative explanations were possible. In the absence of Einstein's theory (which did not exist when the contraction hypothesis was mooted), these would have been not only possible, but by far the most likely, explanations. Hence the Kennedy-Thorndike experiment could not have falsified the contraction hypothesis.

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[1] W. Pauli, *Theory of Relativity*, London, 1958, pp. 5-9

[2] The Russian original of this paper 'On the Direct Experimental Verification of the Second Postulate of the Special Theory of Relativity' appeared in *Optika i Spektroskopia*, 1960, **9**, 134-135. An English translation *Optics and Spectroscopy* is published by the Optical Society of America. I am indebted to Professor A. Janis for this reference to the work of A. M. Bonch-Bruevich.

[3] At this point, Bonch-Bruevich cites two earlier papers of his in *Optika i Spektroskopia* for 1956 and 1957 respectively, and another paper of his in *Doklady Akad. Nauk*, 1956, **109**, 481.

[4] *Proc. Amst. Acad.*, 1904, **6**, 809

That is really all there is to say about the matter. The rest of Professor Grünbaum's discussion on this point is mere verbiage.

It might be added that the Lorentz transformation (based on FitzGerald contraction and time dilatation together, as distinct from contraction alone) was an *ad hoc* attempt to save the Maxwell-Lorentz electromagnetic theory, but not an *ad hoc* attempt to explain any particular experiment—certainly not one whose result was published in 1932. It could be falsified by a falsification of the electromagnetic theory, such as would be provided, for example, by an experimental demonstration that the velocity of light is c with respect to its source alone. This was a part of Ritz's hypothesis.

With regard to my paper on that hypothesis, its purpose was to show that 'the critique given in W. Pauli's careful treatment' was inconclusive. I think this is obvious when once it is pointed out, and no-one has attempted to question it. In so far as Professor Grünbaum rests his case on Pauli's treatment, therefore, its situation is precarious. I am indebted to him, however, for calling my attention to Bonch-Bruevich's experiment, of which I had not heard. The use of opposite limbs of the Sun instead of a double star overcomes the defect in de Sitter's test, since the velocity of the sources does not change appreciably during the passage of the light, but from the account in *Physics Express* it is clear that it has no bearing on Ritz's hypothesis because it presupposes what it is held to prove. The times t_1 and t_2 are not measured kinematically, but calculated from the interpretation of the formula $c = n\lambda$ given by the Maxwell-Lorentz theory. But that theory implies that the velocity of light is independent of the motion of the source, so it is illegitimate to use it in this connection. Only a purely kinematical measurement of the times will do, such as the process I suggested in the *Nature* letter which Bonch-Bruevich mentions. I have gone into the whole question of the relations between electromagnetism and kinematics in *Philosophy of Science*, 1960, **27**, 233; there I wrote, for example, 'there can scarcely be a doubt that, so long as we remain within the framework of present electromagnetic theory, we shall find complete consistency with the Lorentz transformation, for the electromagnetic equations themselves are invariant to it'. Bonch-Bruevich's experiment merely exemplifies this statement.

HERBERT DINGLE

REVIEWS

CHALLENGE TO DUALISM

MANY a theoretical physicist has toyed with the idea of a unified, comprehensive theory combining cosmology, quantum mechanics, and relativity. However, cosmology as well as quantum physics abound with an increasing amount of intrinsic difficulties, incompatible traits, "messy" facets, conflicting hypotheses and gigantic extrapolations. To cite a few of our present worries:

(*a*) In cosmology we may choose between relativistic world models (static or expanding, expanding or contracting, or both); the steady-state; the electric universe (positive charge-excess).

(*b*) In current physical theory we seem to have the choice of macrocausality, microcausality, or acausality. The interaction of elementary particles requires a tentative causality postulate (elimination of infinite constants). Does causality mean that every effect has a cause? That there will be no absorption before emission? Is causal interdependence only an ordering in time? Or only local interaction (field theory)?

(*c*) Negative probability may not be a nightmare for a pure mathematician, but for the physicist it presents a fountain of unintelligibility.

(*d*) Bohm's conjecture of a sub-quantum level where $\delta x \delta p < h$ (in analogy to $\delta p \delta x < ma$).

(*e*) The obscurities concerning renormalization in quantum electrodynamics.

(*f*) The property of the δ-function that it is infinite at one point (where it is not zero), yet approaches infinity in such a way that its integral is unity.

(*g*) What is a quantum-mechanical formalism? Is it not a fact that quantum mathematics is already an *interpreted* formalism?

(*h*) The aphysical so-called functional representation of corpuscles and its relation to the " onde moyenne " (Destouches and Aeschlimann).

This list would be incomplete without reference to the most powerful tenet in quantum theory: the wave-particle duality. It is the merit of one of our foremost quantum theorists, Alfred Landé, to have challenged the validity, the very plausibility of that duality being a fundamental, genuine characteristic of nature.

Landé's *From Dualism to Unity in Quantum Physics*[1] is an exciting, crucial, though highly controversial, piece of incisive reasoning, in part truly original—a rare instance of independent yet responsible probing into some of the " mysteries " in quantum theory.

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[1] Cambridge University Press, Cambridge, 1960. Pp. xvi + 114. 18s. 6d.

Landé proposes to show that wave-particle duality is not an immanent property of the micro-realm of matter. He does not deny that complementarity procedure interrelating corpuscular and undulatory phenomena is legitimate and even useful. But he refuses to accept the prevalent viewpoint that wave-model and particle-model can be treated on a par, as if we encountered here two equivalent aspects of certain physical entities. In accord with Born's statistical interpretation of the ψ-function he pleads for a unitary quantum mechanics of particles and he contends that such an exegesis applies equally well to discrete corpuscles as to field oscillators, to the neutron no less than to the vibrating field. " Actually, an electron always behaves exactly as a particle ought to behave. It is a myth that it sometimes misbehaves." Hence it is sheer solecism to conceive of the " real " particle being also a " real " wave: parity between the concrete and an abstraction is muddled reasoning, at least in an exact science.

No doubt, Landé—who presents his case with the acridity of a " young man in anger ", with logical rigour and the inexorable spirit of a crusader—anticipated many of the counter-arguments of the adherents to the customary duality standpoint. Thus, he does not question Born's qp-commutation rule, qp-periodicity being wave-*like*, Schrödinger's p-operator rule, and, most important of all, the amazing wave-like relation between co-ordinates and momenta, de Broglie's quantization $E = h\nu$, $p = h/\lambda$. Neither does he deny the veracity of experimental results. Yet he does not regard quantum rules as mathematical expressions of duality, uncertainty, and complementarity; according to him these rules can be explained in a simple manner by recourse to postulates of invariance, symmetry, the law of unitary transformation, and other similar exact principles; in brief: by invoking non-quantal " axioms ".

Could one marshal authoritative support for Landé's claim that the " wavicle " dualism is a recondite concept, alien to common sense, a specious philosophic profundity? Well, de Broglie himself preferred the expression of a wave being *associated* with a corpuscle. And Schrödinger stressed again and again that the wave-picture is neither the observed facts nor nature; indeed, wave-picture and observable facts are not even in one-one correspondence; we must *think* in terms of waves, but the observed pattern manifests itself in the form of single particles. But he also suggested that particles are illusions, generated by high crests of waves in a continuous medium, " explosion-like events within the wave-front ": the unitary wave interpretation.

Although I do not wish to indulge in analytic word-spinning, I feel that Landé's insistence upon appraising physical issues exclusively in the light of objective theory cannot conceal the fact that he is forced to argue ontological issues. Electrons and electric fields are real things no less than trees, whereas ψ-waves are mental abstractions? All right, this is a reasonable viewpoint. But it represents a philosophic position; for instance, Popper with whom Landé is in strong agreement on vital questions concerning philosophy of science regards fields of forces as " metaphysical " and he has given good reasons for his assertion. Landé too is philosophically committed; his approach to realism in physics definitely makes sense. Somehow it appears as if he is wary of any explicit " confession of faith " in order not to violate the simple rules of " orderly thinking " in regard to quantum theory.

It is almost a painful task to have to concur with Landé's conclusions that dualism is based upon a grave epistemological blunder, namely upon equating a thing, for example an electron, with a property (wave-like statistical distribution of many electrons). If I am a man, I need not fear a black cat; if I am a mouse, then an encounter with this cat might be most unpleasant. Likewise, if I accept a unitary particle interpretation, then the wave-attribute will not do me any harm; if I am a dualist, then I must become prey of the 'real' matter-wave! However, when one studies Landé's method of demonstration, one has to admit that he does not just offer fantastic conclusions, but a concatenation of neat grounds why his main thesis can demand approval. Quantum theorists used to approach the particle-wave issue in the form of a *modus ponens*: from $p \supset q$ and p to infer q, but they could not reconcile this inference with the equally valid canon: from $p \supset q$ and not-q to infer not-p (*modus tollens*). This scheme is grossly oversimplified and therefore vulnerable, but it might illustrate an attitude. The gist is that Landé is right in emphasising that the working physicist regards the electron as a discrete particle, that is, as p in the above scheme.

A considerable section of the book is devoted to the *irreducible* statistical character of quantum events. Landé does not treat von Neumann's proof as valid; moreover, he urges that there is no need for such a proof at all, since Born's statistical particle interpretation is self-explanatory and self-sufficient. Von Neumann first evolved a mathematical formalism (?) and later endowed it with a physical meaning. But, Landé avers, any given formalism permits a variety of interpretations. Of course. But von Neumann only proceeded in a manner which is frequently practised; to wit, Green's theorems and functions (e.g. Dirac), Kronecker's δ_{ij} (a purely formal concept, for no such function actually exists), abstract algebra etc., while von Neumann faced a concrete situation and applied a " formal " method to eliminate the *ad hoc* nature of certain traits in quantum theory. I do not see any difference between his approach and that of Carathéodory. In fact, Landé himself, as will be presently shown, could not do without such aids in order to drive his anti-dualistic message home. In parenthesis: I do not concur with the view that von Neumann's proof is circular and thus invalid (or useless); on the contrary, I regard it as a paradigm of that inferential rigour which one can muster in this type of enterprise.

Landé's armoury for combating duality consists of the postulates of symmetry, invariance, reproducibility, the law of unitary transformation, of cause-effect continuity, and the necessary conformity between statistical fact and *a priori* theory. Let us arbitrarily commence with the law of cause-effect continuity.

Leibniz enunciated three kinds of continuity. It was an ingenious move on Landé's part to invoke that variety which postulates: when cases (antecedents) continually approach and are finally merged in each other, the events (consequents) must do so too. Now, this is a mathematical principle and Landé renders it plausible that it also works in physics (as Leibniz anticipated). This postulate is mobilised to justify the occurrence of cause-effect continuity together with discontinuous acausal events. In other words, between two states M and N are intermediate states " sometimes M " and " sometimes N ". In short: there are indeterminate cases governed by statistical averages, where the continuity of cause-effect is *a priori* incompatible with determinism. Landé introduces the notion of " fractional equality " which, owing to Leibniz's principle, is supposed to bridge the dichotomy between equality and inequality, so that $M \sim N$. On this showing, utilizing the Stern-Gerlach " splitting-effect ", combining it with the postulates of symmetry and reproducibility regarding a test result, he explains the strange quantum phenomenon that a separator or filter, constructed to pass M-state particles, does in fact pass N-state particles: the N-state particles jump, because they are in contact with the separator, to the new state M and can therefore pass. In analogy, the rejected N-state particles jump to a non-M state so that they surrender their passage tickets. Without adducing Planck's *h* or Reichenbach's three-valued logic, the forementioned postulates and the splitting effect suffice to show why there are states intermediate between " passed " and " blocked "—states which are sometimes passed, sometimes rejected by the M-instrument (or N-filter, as the case may be). Heisenberg's *qp*-uncertainty becomes thereby a special case and need not be regarded as a *specific* quantum effect! The symmetry requirement leads to $P(M \rightarrow N) = P(N \rightarrow M)$ so that one arrives at the probability of transition from state M to state N in an N-test and from N to M in an M-test. One has here a statistical counterpart of the two-way symmetry relation, viz. reversibility in classical mechanics (deterministic). The physics involved in Landé's reasoning seems to me sound, but I wish he had chosen a more appropriate term for " fractional equality ". The logic of this expression is incondite and vulnerable.

He provides some convincing additional arguments in support of intransigent indeterminism and acausality; Planck's insistence upon statistical regularities being reducible to dynamic, accurate and absolute, that is deterministic, laws holding for the individual event, is cogently repudiated. Whence the conformity between formal inference and individual facts in statistical ensembles? Must we not admit a " miracle " of co-operation? Landé maintains that this state-of-affairs is irreducible—it is a primitive property of physical nature. That is, if a miracle is an ever recurring experience, then one might as well accept it without bewilderment and christen it, say, Gauss error law. And yet, that there is no causal explanation for the omnipresent correlation between statistical dispersion and random theory, is a corroding issue, if only for deeply entrenched psychological habits—an observation readily granted by Landé. I think that the search for sufficient causes for every event is indeed foredoomed, *provided* one is resigned to the idea that some essential features of quantum mechanics are definitely ultimate.

When dealing with the metric of probabilities, he makes use of a so-called law of unitary transformation which he equates with the law of interference of probabilities in quantum theory. He proposes to furnish rigorous proof combined with sound physical postulates

> why the statistical manifestations of particles should obey a law of " interference of probabilities " *via* a complex-imaginary amplitude called Ψ.

And also, why co-ordinates and momenta of particles can be depicted in a periodic wave-like relation. This topic suffers from a too condensed treatment. Further, there is no such " law ", but a beautiful theorem or rule in linear algebra applying to hermitian and quadratic forms. Linear transformations A which possess the property $G(Au, v) = G(u, Av)$ (let $G(u, u)$ be the primitive form), lead, under strictly defined conditions, to linear transformations U which leave invariant the fundamental hermitian form $G(u, u)$; those U transformations are designated as *unitary*, or in the real case, as *orthogonal*. Again, under special conditions we can obtain a complete orthogonal system of vectors, which can be normalized, rotated, etc. We are interested in the fact that the matrix is unitary and that for linear mappings any algebraic relation between matrices is invariant under certain transformations.

There is an old story about a Russian peasant who visited Moscow for the first time and landed at the Zoo where he noticed the giraffes. " Look, Mother," he whispered to his wife, " look what the Bolsheviks have done to our horses ". Well, I felt like this peasant when I read Landé's interpretation of magic squares, unitary transformation, and metric of geometrical structures. However, on very patient scrutiny it must be conceded that his derivation of P-correlation is a beautiful and flawlessly reasoned piece of work, in its elegant deductive procedure a true model for analytic thinking in theoretical physics. I only wish he had not compressed his arguments in such a tight fashion: the " unpacking " of the diverse claims is at times true labour. If P is the probability of transition and P-tables are appropriately conceived of as unit magic squares, then by correct application of unitary transformation and the assumption that ψ is a matrix ($P = |\psi|^2$), one must arrive at an expression demonstrating with mathematical necessity why we have interference of probability amplitudes ψ. Landé is unduly worried about unitary transformation being the only possible correlation law linking P-matrices; a uniqueness proof for the monopoly of unitary transformation in regard to P-correlation law might probably be attainable, on sheer symmetry (or group-theoretic) grounds. Still, as long as the validity of approach proposed by Landé is not disproved, his arguments hold on mathematical *and* physical evidence. I suggest that equation (2*b*) on page 45 be altered so that it contains the explicit mention of *unit matrix* to avoid possible confusion.

Perhaps the most valuable and consequential insight gained by Landé concerns a feasible, unambiguous interpretation of the Schrödinger wave equation. One sometimes wonders whether the ψ-function will ever exhaust the creative (often truly poetical) minds of imaginative physicists and philosophers. That poor function or wave is supposed to occupy a definite region in space; lacks location even in fictional space; is a pilot wave; a quantum potential; guides events; gradually expands and occasionally shrinks suddenly; contains no physics at all, etc., etc. Do you remember what that suave gentleman replied when reprimanded for having made some derogatory remarks about a lady-friend? " Granted, there is a lot to say in her favour, but the 'other' is more interesting." Indeed, some flagrant absurdities appealed to physicists for various reasons and Landé fulfilled a necessary " therapeutic " task by eschewing the mere *interesting* readings into the ψ-function.

The wave equation is not a deterministic equation of motion; $\psi(r, t)$ does *not* represent a physical state in space varying with time; Bohm is right when he stresses that the Schrödinger equation depends upon the representation one uses; however, it is misleading to depict the ψ-function in three-dimensional space; hence this kind of "model" ought to be excised from our picture-store in quantum mechanics. It is the mathematical formalism of quantum mechanics that determines, through the square of the modulus of the complex function $\psi(q)$, the probability that a measurement on the system (particle) will produce the values of the co-ordinates to be in the element dq of *configuration* space. The normalization condition for ψ-functions, $\int |\psi|^2 dq = 1$, follows when we integrate $|\psi|^2$ over all configuration space, for we lay down, by definition so to speak, that the sum of all the possible probability values of the system's co-ordinates should be equal to 1. Landé proves, adopting a different approach, that a complex imaginary function $\psi(q, p)$ is the periodic probability amplitude function $\psi(q, p) =$ const $\exp(2i\pi qp/h)$ and is equivalent to the Schrödinger rule and the Born commutation rule; Heisenberg's uncertainty relations (holding for every pair of canonical conjugate variables, not only for q and p, but also for E and t etc.,) can easily be obtained from the exponential form $\exp(2i\pi qp/h)$

and exp $(2i\pi Et/h)$ for $\psi(q, p)$ and $\psi(E, t)$ respectively. Landé's list of six current views as to the " correct " interpretation of the wave-function could become a collector's item displaying inconsistency in reasoning among men of science.

Schrödinger had, at some stage, uttered his doubts as to his unitary wave interpretation and also whether ψ-waves have physical existence of their own. But he died irreconciled to Born's statistical interpretation and thus to the notion of transition probability. He believed that the latest findings conform neither to the corpuscular nor to the wave theory—an observation which is reflecting our present ignorance regarding interaction of elementary particles; but he applied his scepticism to customary dualism itself, not only to quantum field theory. That neither the particle nor the wave " picture " can claim truth was his final agnosticism. Landé is right: it can be demonstrated that the Schrödinger equation is a probability metric; after all, a curve is not a " real " thing. Jeans' " waves of knowledge ", the recourse to the observer's " state ", the cryptic allusions to " material waves "—all these startling expressions are examined by Landé and found wanting. The wave is the statistical " disposition " of the particle, and wave-*like* phenomena are the direct consequences of non-quantal postulates.

I fully concur with Landé when he pinpoints the defects of a unitary wave conception; it could never account for corpuscular phenomena. On the other hand, Born's statistical particle theory permits us to explain the periodic wave-like relation between co-ordinates and momenta. I remember how Schrödinger enchanted us with the simile of the violin and its sound to illustrate the particle-wave distinction. It made sense, at that time, but Weizsaecker's dualism of two " pictures " which end in an abstract formula can be shown to be an " emergency exit "—the product of a facile positivistic defeatism. How come, one asks, that Born himself did not pronounce his original unitary·theory with the same zest and power of conviction of Landé, all wave and duality talk notwithstanding? That " real " things are no more than *Gestalten* or invariant constants must lead to mathematical idealism, to a flight into a-physical realms. Is it possible that Born has always been right in his attempt at deriving quantum mechanics from classical probability considerations? In other words, has Landé proved that the Born of yesterday has to be protected against the Born of today? Before we can answer this question, let us glance at the obvious reason for Born's reorientation towards Bohr, Heisenberg, etc. No doubt, it is quantum field theory (relativistic) and the intractability of bosons (e.g. photons, mass ~ 0, spin 1). Photons are not localized and should not be treated as particles at all. Now, bosons obey the Einstein-Bose statistics with symmetric state-functions and cannot be subjected to Boltzmann's classical statistics; in contrast the Fermi-Dirac statistics (i.e. Pauli principle) with anti-symmetric state-functions applies to fermions, e.g. electrons, with spin $\frac{1}{2}$. In the latter case too is Boltzmann statistics without value. Schwinger stipulated that fields with integer spin *must* obey commutation relations and fields with half-integer spin anti-commutation relations. (See also Lüders' theorem). Since the quantum theory of fields seems to permit the possibility of different types of fields, the symmetry principle cannot be used as a master key any longer. Can we reasonably assume that a field may possess different ψ-functions from that of the form $\exp(2i\pi qp/h)$ which Landé selected? I think that his general formulation of the wave-function cannot be invalidated, *provided* one employs a notation such as $\psi(p, q)$, $\psi(K, q)$, $\psi(E, q)$ in a consistent manner; we need the general expression for the operator p and all the diverse functions $\psi(G, q)$, for any arbitrary operator $G(p, q)$ can then be computed. Born is concerned about the grave fact that quantum theory does not suffice to explain the field properties of fundamental particles and about the question whether conservation of energy will still obtain in the new theory, which may or may not be quantized. However, Landé emphasises that he does not wish to deal with this big problem of relativistic field theory. His goal is the deduction of quantum mechanics from non-quantal propositions and a sharp definition of the wave-function. One could put it this way: he confines himself to intrapolation within quantum mechanics proper and shuns bold extrapolations. This self-imposed restriction renders Landé's case more convincing even to a carping mind. The issue at stake: can we afford to ignore relativistic quantum field theory when discussing the particle-wave duality? I think his assertion that this is possible has been vindicated by his arguments.

As to be expected, Landé cannot accept the Copenhagen interpretation of quantum theory. Any subjectivism in physics is anathema to him and it is interesting to note that Schrödinger, in one of his last papers, also roundly condemns the language of Copenhagen epistemology. I do not wish to dampen Schrödinger's and Landé's polemic spirit in this matter, but it seems to me as if the opponents of that interpretation have not taken the trouble to study the so-called Copenhagen commitment in detail. N. R. Hanson's attempt to remedy this omission did us a salutary service by showing that only a cautious analysis of that viewpoint can keep the discussion free from misapprehensions and inappropriate claims. Bohr and Heisenberg could persuasively argue that their ideas need not result in a physics of solipsism, if one plays the game according to their set of rules! In brief: Landé is right in questioning the validity of duality being an intrinsic feature of nature, but he can hardly deny the descriptive value of that term within a clearly defined frame.

When did duality as a tantalisingly inadequate expression give its first performance? The common opinion is that de Broglie " injected " wave-particle dualism into theoretical physics. Landé corrects this wrong assumption: Einstein's hypothesis concerning photons and Duane's corpuscular theory of X-ray diffraction paved the way for the wavicle hybrid. However, current knowledge will compel us to revise our appraisal of those two classic contributions, so that the spurious foundations for duality will be placed in proper perspective.

Voltaire remarked that the secret of being tiresome is to tell everything. To my mind, Landé is aware of the myriad ramifications of his unitary particle interpretation, but he had to concentrate upon some essential aspects and propound them in such a manner that the core became lucid. Will his radically developed Born theory imprecate imminent doom on " wavists " or " waviclists "? I can only hope so, since I believe in the self-correcting process of scientific method. Landé is no iconoclast—he belongs rather to that rare type of creative scientists who engage in excoriating the arguments of co-workers until their theoretical edifices tumble down. Planck, Einstein, Bohr, de Broglie, Born, Heisenberg, Schrödinger, Jordan, Landé—they all " did " physics, they were creative in their respective domains, and then they pondered over the physical, the epistemological significance of their discoveries and later on physical theory in general. The logic, the method responsible for the formulation of physical propositions is often alien to them. Landé is less guilty of that marked shortcoming.

I found the book under discussion a fascinating piece of powerful argumentation, a true requiem for duality, if not a solemn one, then at least an honest one. There are some typographical errors, but they do not become irritating; still I wish that a new edition would display at some detail intermediary steps in order to spare the reader the bother of searching for necessary links. The discussion on reproducibility does only in part support Landé's main thesis; nor is the thorny problem of the discernible individuality (?) of a particle satisfactorily dealt with. Yet the analysis of the meaning of the term " state " in quantum physics is an example of precise and relevant elucidation. Similarly, Landé succeeds in illuminating the most important implications of the Second Law, consequences often ignored or wrongly presented. Here too one cannot impute to him " wisdom of hindsight ", as he had enunciated his views long before they became either fashionable or neglected; and also here is the affinity with Born evident in all vital aspects.

The saga of duality is recounted wherever theoretical or nuclear physicists pursue their studies, their teaching, their research. I hope, I urge, that Landé's book will be read by them, argued *after* his arguments have been listened to. Landé is no Don Quixote . . . and the castles of duality are not even " real " windmills.

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The Biological Way of Thought. By Morton Beckner.
Columbia University Press, New York; Oxford University Press, London, 1959. Pp. viii + 200. 48s.

THIS book is a welcome addition to the classics of biological methodology. It is a worthy successor after thirty years to J. H. Woodger's *Biological Principles* to which it owes much as a pattern and starting point. Not least in this connection is its main aim which is to examine the ways in which concept formation and explanation in the biological sciences differs from those in the other natural sciences. The author has drawn widely from other writers in the philosophy of science and the work might be regarded as a restatement of *Biological Principles* with the benefit of the writings of contemporary logicians and methodologists upon which to draw. It is perhaps a pity that one writer which the author has ignored is Popper. However, there is much that is original in the book, and the clarity of writing and the precision of many formulations of ideas and problems makes the book lucid, thought provoking and a distinguished contribution to the subject.

The chapter on the Types of Biological Concept is a key to the subject matter of the whole book and much of what follows later depends on the validity of the ideas in this section. The author distinguishes between concepts which can be well-defined (W-defined) and those which can also be effectively defined (E-defined) and this distinction is skilfully used in subsequent discussion. The conditions imposed on a W-definition are intended to be an application of the verifiability criterion of meaning. They are that

1. A test-procedure is describable for determining whether an entity is a member of the extension of the concept being defined.

2. The test-procedure consists of a finite number of logical and/or physical operations.

3. Each operation of the test-procedure is compatible with the laws of nature and of logic.

A concept is E-defined with respect to a non-empty class K of entities if it is W-defined, if K is equal to or is included in the extension of the concept, and a test-procedure is describable for deciding with a high degree of probability that an entity is a member of K, and if this test-procedure, known to be compatible with the laws of nature can be carried out at present. When the class K and the extension of the concept coincide the concept is E-defined. The concept of 'gene locus' is W-defined, but not E-defined. The author gives the concept of 'prospective potency' in an embryo as being E-defined but not W-defined. It is not easy to see how this can be from the definition of the E-defining relation where a W-definition appears to be an essential requirement of the E-definition.

From these notions we find that there are three types of concept in biology which in one sense are fully unique in biological theory.

1. Polytypic concepts:

This class of concepts is defined with reference to a set of properties $f_1, f_2, f_3 \ldots f_n$ so that each individual in the class possesses a large number of the properties, each f is possessed by large numbers of the individuals and no f is possessed by every individual. This is not the same as the Boolean product of all the properties, and the author discussed the significance of the difference. It appears that this vagueness in the polytypic concept is its value. Such concepts have important uses in any discussion involving systematics and in the analysis of other types of explanation which are discussed later in the book.

2. Historical concepts:

Criteria are given for their use which, briefly, indicate that a process is always involved, and that the process is describable as a temporal sequence of stages. The term ' hybrid corn ' is an example of an historical concept.

3. Functional concepts:

' A concept C is " functional " if, and only if, C is W-defined (. . .) such that every application of C to a system s and process F is (logically) false unless there exists a system s' and a process F′ (other than s and F) and at least one environment and state of s' in which s' does exhibit F′ and in which s' would not exhibit F′ if (1) F failed to occur, or (2) s is not a part of s' or that environment.' The names of most organs are functional concepts together with the names of responses and so forth.

Before discussing the rôle that these concepts play in biological theorising, the author discusses different types of model found in biological theory. After distinguishing ' model = interpretation of calculus ', the ' mathematical model ' and ' replica = model ' Professor Beckner draws attention to a fourth type, an example of which he finds in the work of Rashevsky. Other examples are the well-known biophysical models for cell division, the evolutionary models of Fisher, Sewall Wright, etc., the neural models of McCullock and the feed-back models of the cyberneticists. They are a technique of explaining or quasi-explaining a class of phenomena (called the field) by references to properties of systems (called the subject) which are either parts of the system exhibiting the field phenomena or of its environment.

I do not find it easy to see the point of the introduction of this fourth kind of model or why it should be called a model at all. Its nature appears to be that of an explanation of events at one level in terms of parts of a lower microscopic level. Beckner points out that there are two assumptions characteristic of his models. These assumptions are, firstly, law-like statements about the subject of the model and, secondly, existential statements about the existence of elements of the subject which simplify because they serve to reduce the structural complexity of the field. This seems to me to be characteristic of any change of explanatory level. The whole tenure of the argument becomes apparent when one realises that Beckner is accepting Hempel and Oppenheim and Braithwaite's requirements that for them an explanans must be true. By adopting this criterion of the satisfactoriness of an explanation, he wishes to use the concept of model in which the explanans may be known to be false or certainly not known to be true. He then talks of his models as being quasi-explanations. Much of what he says about levels of generality and the reducibility of theories has been said before in, I feel, clearer terms. It is a pity in this context that he nowhere refers to Woodger's description of the way in which theories have levels, which is found in the early parts of *Biology and Language*. Woodger's description is, to me, much clearer, and nowhere conceives that the statements of the higher level are necessarily true. Indeed, if this were the case, we should never progress in science by providing alternative hypotheses.

I have described the keystones of Professor Beckner's views in some detail because I hope it can be seen from my account how important these topics are for any analysis of biological problems such as are found in Systematic Theory, Evolutionary Theory, or in Organismic Biology. It is impossible to review in detail the analysis he offers in these various fields, and I shall only comment to give some indication of the approach he adopts. On one trivial point of terminology I think it is a pity he has used the term genetic analysis in the sense of historical analysis. It is used clearly and explicitly with this meaning as opposed to the technical biological sense. Philosophers and psychologists will be quite familiar with this usage; on the other hand, students of the biological sciences will not, and with a well-developed science of the same name at their finger tips, will find the book confusing until they have become accustomed to this other usage.

In both the chapters on Systematics and Genetic Analysis polytypic concepts play an important rôle. One is left wondering whether the apparent success of the concept is as real as it appears. It is the sort of concept for which one feels intuitive sympathy, but the proof of its success is in its use in statements. I would recommend that a detailed instance of its usefulness be worked out. It is easy to use the concept for an intuitive analysis and to feel that ' in principle ' it should be satisfactory. However, its logic is only discoverable by taking a piece of evolutionary theory and seeing whether statements involving the concept generate the sort of consequence that is desired. It is a pity that the author did not do this in the book.

The next part of the book is a discussion of the rôle of functional concepts in teleological explanations. The writings of Braithwaite, Sommerhoff, and Nagel are discussed. In particular Beckner points out that Sommerhoff's analysis in terms of directive correlation always involves the success of the object in reaching its goal. Any teleological explanation needs to give an account of goal-seeking behaviour in those cases where the system is only partially successful—of how the object gets onto the target even if it does not score a bull.

The later chapter on selection theory and organismic biology involve a synthesis of the use of functional and historical concepts with model-explanations. The methodology involved in part-whole relationships is discussed quite fruitfully. A detailed discussion of the points raised would require another book. I am sure that in departments where biological methodology is taken seriously this book will generate ideas for many years to come.

R. F. J. Withers

Isaac Newton's Papers and Letters on Natural Philosophy and Related Documents.
Edited with a General Introduction by I. Bernard Cohen assisted by Robert E. Schofield.
Cambridge University Press. 1958. Pp. 501. 70s.

In this book Professor Cohen has assembled all Newton's published papers and letters relating to optics, chemistry, and other branches of natural philosophy which were issued either during his lifetime or shortly after his death. This volume may therefore take its place on our bookshelves beside the *Principia* and the *Opticks*, for the three together can be regarded as ' the complete corpus of Newton's writings on physical science that actually influenced the scientists and thinking men of the " Age of Newton " '. In addition to works from Newton's own pen, the editor has included Halley's review of the Principia and his account of Newton's theory of the tides which he wrote for the benefit of James the Second, Fontenelle's obituary notice (in English) of Newton and the Seventh and Eighth Boyle Lectures delivered by Richard Bentley in 1692, being ' A Confutation of Atheism from the Origin and Frame of the World '. The book ends with a useful Appendix of references to Newton in Birch's *History of the Royal Society*. The different sections are prefaced by contributions from several American scholars: Thomas S. Kuhn on the optical papers, Marie Boas on the chemical papers, Perry Miller on Bentley and Newton, Robert E. Schofield on Halley and the *Principia*, and C. C. Gillispie on Fontenelle and Newton.

Nearly half the book is devoted to optics. Almost all the papers on this subject were originally published in the *Philosophical Transactions*. As Dr Kuhn points out in his introduction to these papers, Newton was the first great scientist to use the then new medium of the journal article to give an account of a new branch of experimental science. Moreover, it was his work that first stimulated international debate in a scientific journal. These papers and Dr Kuhn's discussion make it clear that the essential difference between Newton and other contemporary students of optics was his great success in interpreting his experiments mathematically. On the other hand, his skill as a practical designer of optical instruments was not particularly remarkable, and indeed he was often wrong in his suggestions. His celebrated reflecting telescope remained essentially a curious toy for half a century, until James Hadley succeeded in grinding a parabolic mirror.

Newton's achievements as a chemist are assessed by Marie Boas who emphasises that a full understanding must still await a careful analysis of the extant chemical notebooks in the University Library, Cambridge. Despite the scores of alchemical works in his library, Newton's approach to chemical problems was close to that of Boyle who had no use for mysticism in science. Underlying Newton's chemistry, no less than his physics, was the corpuscular hypothesis of matter. But, although the 'mechanical philosophy' endeavoured to explain all chemical and physical properties of bodies solely in terms of geometry and the motion of particles, Newton—unlike Descartes—felt obliged to retain one ostensibly occult influence which he at first called 'sociability' and later 'attraction'. Nevertheless, he tried, as far as possible, to eliminate its occult overtones. His notorious disavowal of hypotheses was influenced by his sensitivity on this score.

For the student of the philosophy of science, Newton's correspondence with Bentley and its lucid analysis by Perry Miller will be of particular interest. The contrast between the great literary scholar and the even greater natural philosopher is nicely pointed in the Third Letter where Newton remarks that 'Those things which Men understand by improper and contradictious Phrases, may be sometimes really in Nature without any Contradiction at all.'

A word of praise must be given to Bernard Cohen for his urbane General Introduction in which he argues that the papers and letters assembled in this volume clarify one of the major Newtonian problems: why did Newton maintain that he was formulating no hypothesis concerning the mechanism of gravitation, and yet at the same time refer (in the General Scholium at the end of the Third Book of the *Principia* in the second edition of 1713) to the possible rôle of the elastic aether as the agent of gravitational attraction? In the early optical papers, in the famous letter to Boyle and in a long statement read many years before to the Royal Society, Newton repeatedly discussed this hypothesis. Cohen claims that, although Newton refused to admit an *untestable* hypothesis into the framework of the *Principia*, he nevertheless wished to remind the cognoscenti that he had framed long ago an elegant hypothesis that might account for gravitation and indeed for other forces of nature.

All those concerned must be warmly commended for producing so useful and timely a volume. Some readers may regret the decision to reproduce each document in facsimile from the original publication, in order to present to the modern scholar the 'very works' studied during Newton's lifetime and in the decades after his death. This inevitably makes the book harder to read and gives it an uneven appearance. But this is a minor objection to so welcome an addition to the literature on Newton.

G. J. WHITROW

The Evolution of Living Things. By H. Graham Cannon, F.R.S.
Manchester University Press, 1958. Pp. x + 180. 12s. 6d.

PROFESSOR Graham Cannon has addressed this book to the layman because he despairs of influencing professional biologists. In it he develops three main themes. The first of these is concerned to show that the views of Lamarck have been so misrepresented by biologists as to mask their explanatory value. In the second, explanations of animal evolution based on natural selection and on genetical theory alone are shown to be unsatisfactory in that they cannot, in principle, provide a complete explanation. The third argues that the influence of the environment on organisms is inherited and that this does adequately explain those aspects of evolution for which the author believes natural selection to be inadequate.

The task is approached through a brief historical sketch covering the six thousand years preceding this century. The brevity of this introduction is achieved by a very considerable simplification of the views and concepts described. In the five ensuing chapters the historical and biological information necessary to the first two themes are described with wholly admirable clarity and economy. The field covered is broad. It includes the genetics of singly and multi-factorial inheritance, cytology, natural selection, the inheritance of environmentally influenced characteristics, experimental embryology and finally comparative anatomy and biochemistry. The seventh, and final, chapter discusses evidence in support of Graham Cannon's thesis that Lamarckian explanations are essential for evolutionary theory.

The criticisms of Neo-Mendelian genetics and Natural Selection advanced are familiar but are re-stated with an impelling and admirable lucidity, although in covering so wide a field for an audience innocent of biological concepts it has been necessary to make considerable omissions. One of these omissions is of crucial importance to the development of the third theme. The argument of this last theme involves a lemma in which an attempt is made to show that evolution must have occurred before living things developed the cellular mechanism on which Neo-Mendelian genetics depends. The reader is led to this conclusion through an introduction to the nucleus, to chromosomes, and to genes and is told that living things of the present must have possessed ancestors devoid of chromosomes. It is held that in consequence of this Neo-Mendelian genetics is not an explanation for the evolution of such ancestors. It might be the case that the conclusion is accurate but to arrive at it on the information given is an over-simplification. Neo-Mendelian explanations of inheritance have now moved from the area of cytology to that of biochemistry. It is not the case that living things are known to exist now that do not contain nucleic acid and it is believed to be the presence of this substance that is essential for Neo-Mendelian explanations. Had the argument been based on the present universality of nucleic acid then on the argument presented it would have been necessary to infer the presence of this substance in ancestral organisms and hence the possibility of Neo-Mendelian mechanisms for them.

The theoretical framework in which the arguments are developed is less clear than the admirable accounts of biological data. Throughout the thesis there is the implicit assumption that teleological explanations are valid in biology. Unfortunately there is nowhere an adequate refutation of the view clearly expressed (by authors such as Nagel, in Feigl and Brodbeck, *Readings in the Philosophy of Science* 1953) that teleological explanations play no valid part in biological explanation. Lamarck's laws are invoked as being fundamental for the argument without the part they play in the theory being made clear. These laws are discussed as if they were explanatory hypotheses but the manner in which they are stated is of the form of generalisations of observation records. For example, Lamarck's first law of 1809 is stated as ' The development and effectiveness of organs are proportional to the use of those organs ' (p. 60). This states a functional relationship similar to Ohm's law and suggests no mechanism by which organs might appear anew that does not involve teleology.

In the argument developed in his final chapter Professor Graham Cannon appears to accept the hypothetico-deductive view of scientific method, at least in part. He tells his reader that the principle he postulates has always been put forward in the past as an idea which cannot be tested experimentally. He then interprets his argument in terms of our current physico-chemical concepts. Unfortunately his interpretation requires us to go back to the origins of life, and this we cannot, in principle or in fact do.

F. T. C. Harris

Theories of Scientific Method: The Renaissance through the Nineteenth Century. By Ralph M. Blake, Curt J. Ducasse, and Edward H. Madden. Edited by Edward H. Madden.

University of Washington Press, Seattle, 1960. Pp. iv + 346. $6.50.

Philosophy of science is a comparative newcomer among the branches of philosophy, and some would say that it has not yet, or even cannot ever, become wholly autonomous. In any case it has not yet its canon of classic texts. One therefore approaches this volume with interest to see how the selection has been made. The Preface announces that ‘ Our authors are primarily concerned with these persistently recurring philosophical topics and problems: theory construction, hypothesis, causality, lawfulness, probability, the experimental methods, the uniformity of nature and the rule of succession (the problem of induction), the universality of causality, and the problem of discriminating the a priori and the empirical elements of science ’. The book consists of thirteen chapters, each devoted to a single author, or a closely related group of authors, and each provides an exposition of the author’s views with copious quotations.

The first two chapters deal generally with discussions of scientific method in the Renaissance, including those of Leonardo and the sixteenth-century astronomers. These are followed by four seventeenth-century writers: Bacon, Descartes, Hobbes, Newton; then by a brief account of Hume’s view of causation; and finally by six nineteenth-century writers: Herschel, Whewell, Mill, Jevons, Peirce and Chauncey Wright. This selection leaves one with the somewhat depressing reflection that, at least to the end of the nineteenth century, those who are known mainly for their writings on scientific method are not the most exciting of philosophers. In the whole list represented here, apart from the brief acknowledgment of Hume, the only philosopher in the first rank is Descartes. Is scientific method really so unrewarding as not to attract the best philosophical minds? The question has only to be asked to betray an important shortcoming in the selection: where are the other members of the apostolic succession of the history of philosophy—Locke, Leibniz, Berkeley, and Kant? Has the textbook stereotype of their philosophical positions so overlaid their actual work that no one now reads them to discover what they have to say on the topics with which this book deals? And yet it might be argued that they are more worth reading for their views on theory-construction, hypothesis, and the relation between the a priori and empirical, than most of their nineteenth-century successors. Without them this survey cannot claim to be complete, and the omission is more regrettable since, in spite of all that has been written on these philosophers, good commentaries on their philosophy of science in its contemporary scientific setting are not easily available.

This book would not therefore be a complete introduction to the ‘ classic sources ’, but for those authors who are included it does a workmanlike job. The earlier chapters convey something of the polemics of a period when philosophy of science was the manifesto of the new scientific movement rather than the analysis of established methods. Two problems are introduced: the question of *certainty* and the question of *reality*. We are shown how the Renaissance humanists rejected natural science on the ground that it cannot attain certain truth, and how Descartes and Hobbes, accepting this as a criterion for the value of science, attempted to show that truth in science is attainable and to derive it from self-evident principles. Huygens and Newton are credited with being the first fully to abandon the view that science can be absolutely certain and final. The problem of the real or fictional nature of hypotheses was confused throughout this period with the problem of certainty, and is not always carefully distinguished in some of the comments of the authors of this book, especially in the chapter on Descartes. But the chapter on the Renaissance Astronomers gives an interesting account of the interactions of truth and falsity, and reality and fiction, in their claims for the planetary hypothesis, and in their generalisations of these views from astronomy to physics. Clavius and Kepler both gave comparatively clear accounts of hypotheses as potentially *real* descriptions of the system of the world, having high probability in relation to the evidence, but which may nevertheless be *false*. The problem of certainty hardly arises again after the end of the seventeenth century (although the contribution of Locke on this point might have been included), but the problem of fictional hypotheses does continue to arise, and it is a pity that it is not pursued into the writings of the nineteenth-century positivists.

The chapters on Bacon and Descartes do overdue justice to both these philosophers in showing how in each case their own scientific investigations forced them to modify their initial philosophical positions, even at the cost of inconsistency. Thus Bacon the empiricist and compiler of facts is shown to have in practice a sound idea of the function of hypotheses, and Descartes the rationalist spends much time analysing the role of experience in science. There is a kind of homeostasis about the new scientific method which tends to stability in spite of the exaggerations on one side or the other of its principal exponents, and, as always in a progressive period of philosophy, inconsistency is less important than creative novelty.

Newton's view of hypothesis is a well-worn subject, but it is treated here with detail, balance, and freshness. This account, and also the chapter on Descartes, suggests that our understanding of seventeenth-century philosophy of science would benefit from a study of the use of the terms ' deduction ' and ' induction '. Descartes uses the words almost synonymously in the *Regulae*, and Newton uses such startling locutions as ' deduced from the phenomena '. I have never seen a careful examination of this matter, although with regard to Newton I have a private hunch that he was tempted to generalise the genuine *deduction* of the law of gravitation from Kepler's laws (which he calls *phenomena*), together with his Rules of Reasoning, which are very general assumptions applicable to all phenomena. This reverses the order of deduction of the usual hypothetico-deductive form, and since Newton carries it out in the *Principia* with detailed cross-references to the Rules of Reasoning at each stage, it is plausible to suppose that he thought such a method would be universally applicable to ' higher-level ' theories.

He was not proved wrong until the acceptance of Maxwell's theory, and in the meantime Ampère had made a bold attempt to construct electromagnetic theory on the same pattern.

The interest of the nineteenth-century chapters largely centres in the juxtaposition of Herschel, Whewell, and Mill, and the consequent deflation both of Mill's originality (Bacon's inductive methods were expanded and transmitted to him by Herschel), and of his insight into the nature of theories (much more adequately discussed by Whewell). The chapters on Jevons and Peirce introduce opposing views on the nature of probable inference and its relation to the justification of hypotheses.

In the case of a few of the authors studied here one feels after reading this book that one may well leave it at that—they have historic interest but little to say to twentieth-century philosophy of science. But in other cases one is stimulated to return to the originals, guided by the sign-posts to be found here, and that perhaps is not the least valuable function of the book.

Mary B. Hesse

ANNOUNCEMENT

Meetings of the British Society for the Philosophy of Science

1960

3 October: Professor G. L. S. Shackle, 'Time, Nature, and Decision'
7 November: Dr S. Vajda, 'The Building of Models and the Theory of Games'
5 December: Mr G. Buchdahl, 'Theories and Models'

1961

16 January: Presidential Address, Professor K. R. Popper, 'The Growth of Knowledge'
13 February: Professor E. Gombrich, 'Theory and Observation in the Work of Leonardo da Vinci'
13 March Professor K. Menger, 'A Counterpart to Occam's Razor in Pure and Applied Mathematics'
17 April: Mr P. Alexander, 'Science and Epistemological Atomism'
15 May: Dr E. W. Bastin 'Self-Organising Mechanisms as Models for Scientific Theories'
12 June: Professor M. Munitz, 'The Logic of Cosmology'

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CAUSALITY IN CONTEMPORARY PHYSICS (I) *

M. SCHLICK (Vienna)

Translated by David Rynin †

I *Introductory Remarks*

THE number of conceivable, logically possible, physical worlds is infinite; the human imagination, however, is surprisingly poor in conceiving and working out new possibilities. The power of imagination is so limited by the intuitive conditions of gross perceptual experience that it can hardly by itself progress a step beyond them. It is only by aid of the strict discipline of more refined scientific experience that our thought can transcend its habitual channels. The most colourful fairyland of the Thousand and One Nights is created by a slight rearrangement of the familiar material of everyday life. And upon reflection, when one examines them with more precision, one finds the same to be true of the boldest and most profound philosophical systems: If for the poet it was creation by aid of intuitive pictures, so for the philosopher it is construction by more abstract yet still familiar concepts, from which by apparently more transparent principles of combination new structures are formed.

The physicist, too, at first proceeds in much the same way in the construction of hypotheses. This is particularly indicated by the tenacity of the belief, held for many centuries by the physicist, that to explain nature a copy of its processes in models perceptible to the senses is necessary. Thus, for instance, he repeatedly attributed characteristics of visible, tangible substances to the ether without the slightest reason for doing so. Only when observed facts either suggest or necessitate his use of the new systems of concepts does the physicist realise the new possibilities and break himself free from his former habits of thought;

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* Received 1.iv.60. First published in *Die Naturwissenschaften*, 1931, **19**, 145

† I am much indebted to my colleague Professor Paul Feyerabend for advice concerning difficult passages.

but then he readily and with the greatest ease makes the jump to, say, Riemannian space or to Einsteinian time, to concepts so daring and profound that neither the imagination of the poet nor the intellect of the philosopher could have been able to anticipate them.

The turning point at which recent physics has arrived with respect to the question of *causality* could likewise not have been foreseen. Although there has been so much philosophising about determinism and indeterminism, about the content, validity, and mode of testing of the principle of causality, no one thought of precisely that possibility leading in quantum physics to the key that allows us a view of the real nature of the causal order. Only in retrospect do we realise how the new ideas differ from the old, and we are perhaps a little amazed that so far we have always missed the point. Now, however, after the significance of the concept of quantum theories has been demonstrated by the extraordinary results of its application, and we have had some years to accustom ourselves to the new ideas, it should not be premature to attempt to arrive at philosophical clarity as to the meaning and scope of the ideas that contemporary physics contributes to the problem of causality.

2 *Causality and the Principle of Causality*

The observation that philosophical meditations did not foresee possibilities that were found later on, because of their close adherence to existing ideas, is true also of the ideas I propounded more than ten years ago.[1] Still it is probably not useless to return to some points of those earlier considerations, since in this way the progress accomplished in the meantime becomes so much clearer.

First it is necessary to determine what the scientist actually means when he speaks of ' causality '. Where does he use this word? Obviously, wherever he supposes a ' dependence ' between certain events. (Nowadays it is self-evident that only events and not ' things ' come into question as elements of a causal relationship, since physics forms the four dimensional reality from events, and considers ' things ', three dimensional bodies, as mere abstractions.) But what does ' dependence ' mean? In science, in any case, it is always expressed by *a law*; causality is, accordingly, nothing but another word for the existence of a law. The content of the *principle* of causality then clearly lies in the assertion that *everything* in the world occurs according to laws; it is indifferent whether we affirm the validity of the principle of causality or of *determinism*. In order to formulate the principle of causality or the deterministic thesis, we must first have defined what is meant by a law of nature or by mutual ' dependence ' of natural events. For only when we know this are we able to understand the meaning of determinism, which states that *every* event is a member of a causal relation, that *every* process is wholly dependent upon other processes. (We shall not discuss whether the attempt to make a statement about ' all ' natural processes could lead to logical difficulties.)

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[1] *Naturwiss.*, 1920, 461 ff.

Thus in any event we distinguish the question of the meaning of the word ' causality ' or ' natural law ' from the question of the validity of the principle of causality or the law of causality, and we concern ourselves at the beginning with the first question only.

The distinction we thus make coincides with that made by H. Reichenbach at the beginning of his essay ' Die Kausalstruktur der Welt '.[1] He speaks there of the difference between two ' forms of the hypothesis of causality '. He calls the first the ' implication form '. It is given ' when physics establishes laws, that is, makes statements of the form: " if A then B ".' The second is ' the deterministic form of the causal hypothesis ' [2]; it is identical with determinism, which states that the course of the world as a whole ' remains unchangeable, that with a single cross section of the four dimensional world the past and future are fully determined '. It seems to me simpler and more to the point to characterise the difference involved as the difference between the concept of causality and the principle of causality.

The question then concerns the content of the concept of causality. When do we say that a process A ' determines ' another B, that B ' depends ' upon A, that B is related to A by *law*? What do the words ' if—then ', indicating causal relationship, mean in the statement ' if A then B '?

3 *Law and Order*

In the language of physics a natural process is represented as a sequence of values of definite physical magnitudes. We note already here that, of course, in a sequence only a finite number of values can be measured, that, therefore, experience affords only a discrete manifold of observed magnitudes, and further, that every value is conceived as subject to a certain inexactitude.

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[1] *Sitzungsber. bayer, Akad. Wis., Math.-physic.* Kl. 1925, 133

[2] ' Determinationsform der Kausalhypothese ' in the original (trans.)

Assuming that a large number of such observed magnitudes are given, we then ask quite generally: How does such a number of values have to be constituted so that we may say that it represents a *law-like* sequence, that there is a causal relationship between the observed magnitudes? We may, to begin with, presuppose that the data already possess a natural order, namely the spatio-temporal one; that is to say, each quantitative value relates to a definite position in space and time. It is of course true that only with the aid of causal considerations are we able to indicate the position of events in physical space-time, by passing from phenomenal space-time, which represents the natural order of our experiences, to the physical world. But this complication may be excluded in our considerations, which limit themselves entirely to the realm of the physical world. Furthermore, our considerations are based upon a most fundamental assumption which I here mention in passing only, since it has already been discussed in a previous work (loc. cit. p. 463). It is the hypothesis that in nature there are certain 'similarities', in the sense that different realms of nature are *comparable* to one another, so that we may say for instance: 'the same' *magnitude* that in this place has value f_1 has the value f_2 in another place. Comparability is then one of the presuppositions of measurability. It is not easy to give the real meaning of this assumption, but we need not concern ourselves with it since this last analysis is likewise irrelevant to our problem.

According to these observations, our problem regarding the content of the concept of causality reduces itself to this: what characteristic must the spatio-temporally ordered group of values have so that it may be regarded as an expression of a 'law of nature'? This characteristic can be nothing but again an *order*, and indeed, since events extensive in space and time are already *orderly*, it must be a kind of *intensive* order. This order must be of the *temporal* sort, for, as is well known, we do not speak of causality in reference to spatial order (popularly expressed by 'simultaneous', coexistent events); the concept of activity finds no application there. Spatial regularities, if such there be, would be called 'coexistence laws'.

After limiting ourselves to the time dimension we must, I believe, now say: *Every* order of events in time, of whatever kind, is to be regarded as a causal relationship. Only complete chaos, complete irregularity, is to be designated as an acausal occurrence, as pure chance; every trace of an order would mean dependence, therefore causality. I believe that this use of the word 'causal' is closer to its everyday sense than when confined, as seems to be done by many natural philosophers, to such an order as we could designate by ' complete causality '—by which phrase it appears that something like ' complete determination ' of the event in question is meant (of course, we can express ourselves here in inexact terms only). If we should restrict the word to complete causality, we run the danger of finding no use at all for it in nature, while *in some sense* we do regard the existence of causality as a fact of experience. And there would be even less reason to place the boundary between law and chance at some other point.

The only alternative that confronts us is thus: order or disorder? Causality and law are identical with order; irregularity and chance are identical with disorder.

The result up to the present therefore seems to be: we call a natural process, described by a group of values, causal or regular if the values show any temporal order whatever. This definition becomes meaningful only when we know what is to be understood by ' order ', how it differs from chaos. A most puzzling problem!

4 *Attempts at Definition of Regularity*

It is certain that in our daily life as well as in science we differentiate rather clearly between order and disorder, regularity and irregularity. How are we to understand this? At first glance the answer does not seem very difficult. It appears that we need only make sure how physics actually represents laws of nature, in which form it describes the dependence of events. Now, this form is the mathematical *function.* The dependence of one event upon another is expressed by the fact that the values of a part of the magnitudes are represented as functions of the others. Every order of numbers is mathematically represented by a function; and so it appears as if the desired criterion of order, which differentiates it from disorder, is expressibility by a function.

But as soon as the idea of the identity of function and law is expressed, we see that it cannot possibly be correct. For, as is well-known, whatever be the distribution of given magnitudes, functions may *always* be found that represent just that distribution with any degree of accuracy; and this means that every possible distribution of magnitudes, every conceivable series of values is to be considered as an order. There would be no chaos.

Thus we do not in this way successfully distinguish causality from chance, order from disorder, or succeed in defining rule and law in this manner. As was shown also in our previous considerations, there seems to remain only the alternative of imposing certain requirements on the functions that describe the observed series of values, and by means of them to determine the concept of order. We should then have to say: if the functions that describe the distribution of values of the magnitudes have such and such a definite structure, the represented sequence is in accordance with law, otherwise it is unordered.

Thereby we find ourselves in a rather hopeless situation, for it is clear that in this way arbitrariness is given free rein, and a distinction between law and chance resting upon such an arbitrary basis could never be satisfactory. It could be so only if a fundamental and sharp distinction in the structure of the functions could be ascertained, which at the same time possessed such definite empirical possibilities of application that everyone would immediately recognise them as the correct formulation of the concepts of regularity and irregularity as they are applied in science.

Here there appear simultaneously two ways, both of which men have tried to adopt. The first was already used by Maxwell to define causality. It consists of the following stipulation: the space and time co-ordinates are not to appear explicitly in the equations that describe the sequence in question. This requirement is equivalent to the notion popularly expressed in the phrase: similar causes, similar effects. In fact it means that a process that takes place anywhere and any time in a definite manner will take place in exactly the same manner in every other place and time, under the same circumstances. In other words, the rule states the *universal validity* of the relationship represented. Universal validity, however, is, as has been generally acknowledged, exactly that which in laws of nature has been designated by the ambiguous term ‘ necessity ’, so that it appears as if the essential nature of the causal relation had been correctly hit upon by this stipulation.

Concerning Maxwell's definition of law, which I myself previously defended (in the passage cited), we may say the following:

The concept of law in physics is undoubtedly such that this requirement is always fulfilled. Actually no investigator thinks of formulating laws of nature that refer explicitly to definite positions and moments in the universe. If space and time occurred explicitly in the physical equations, they would have quite a different significance from the one they actually have in our world. The relativity of space and time, fundamental to our world-view, would be denied, and time and space could no longer assume the peculiar role of ‘ forms ’ of occurrence which they have in our cosmos. We should therefore be free to maintain the Maxwellian condition of causality—would it, however, be a necessary condition? We shall hardly be allowed to say that, for surely a world is conceivable in which all events would have to be expressed in formulas in which space and time appear explicitly, without our denying that these formulas represent true laws and that this world is completely orderly. So far as I can see, it would be conceivable, for instance, that uniform measurements of the elementary quantum of electricity (electric charge) would give values for these magnitudes that would fluctuate about 5 per cent in seven hours and then again in seven hours, and then ten hours, without our being able to find the slightest 'reason' for it. And besides that, perhaps still another variation might appear for which we would make an absolute change of position of the earth in space responsible. In this case then the Maxwellian condition would not be fulfilled, but we would surely not find the world to be disorderly and we would formulate its regularity and be able to make predictions by means of it. We shall therefore be inclined to the view that the Maxwellian definition is too limited, and we shall ask ourselves what the criterion of law would be in the hypothetical case we have discussed.

Now, the decisive factor of the hypothetical case seems to be that we could so easily consider the influence of space and time, that they enter into the formulas in such a *simple* manner. If, in our example, the electric charge were to behave differently every week and every hour, or form a completely 'irregular curve', we could of course afterwards represent its dependence upon time by a function, but this function would be very complicated. We would then say that no law exists but that the variations of magnitudes are governed by 'chance'. We do not have to invent cases of this kind since, as is well known, the new physics accepts them as commonplace. The discontinuous events in the atom that Bohr's theory interpreted as jumps of an electron from one orbit into another are regarded as purely accidental, as 'acausal', although we may subsequently think of their occurrence as a function of time. But this function would be very complicated, not periodic, not readily grasped, and it is for this reason only that we say that no regularity exists. But as soon as any simple statement regarding the jumps is formulable—if, for instance, the time intervals become increasingly larger—this would at once appear to us as a regularity, even though time would explicitly enter into the formula.

Accordingly, it appears as if we speak of order, law, causality when the course of events is described by functions of *simple* form; while complexity of the formula is the criterion for disorder, lawlessness, chance. And so one very easily arrives at the point of defining causality by the *simplicity* of the descriptive functions. Simplicity is, however, a half pragmatic, half aesthetic concept. We may therefore call this definition aesthetic. Also, without being able to state what is here meant by ' simplicity ', we must yet affirm the fact that every investigator who has succeeded in representing a series of observations in a simple formula (for instance, linear, quadratic, exponential function) is quite sure of having discovered a *law*, and so the aesthetic definition, as well as the Maxwellian one, obviously discloses a characteristic of causality that is considered a decisive criterion. Which of the two attempts at formulating the concept of law shall we accept? Or shall we formulate a new definition by combining both?

5 *Inadequacy of the attempts at definition*

To sum up: the Maxwellian definition has *in its favour* the fact that all known laws of nature satisfy it, and that it may be considered to be an adequate expression of the proposition, ' similar causes, similar effects '. *Against* this definition is the fact that cases are *conceivable* in which we should certainly acknowledge regularity without the fulfilment of the criterion.

The ' aesthetic ' definition has *in its favour* that it is also applicable to the above considered cases, to which the other one is not, and that also undoubtedly in the prosecution of science ' simplicity ' of functions is used as a criterion of order and law. *Against* it, however, is the fact that simplicity is clearly a relative and indefinite concept, so that a strict definition of causality is not obtained, and law and chance cannot be sufficiently distinguished. It might indeed be possible that we have to take this last idea into account, and that a ' law of nature ' is actually not something so precisely conceivable as one might at first think; however, such a point of view will be accepted only when one is sure that no other possibility remains.

It is certain that the concept of simplicity can only be fixed by a convention that must always remain arbitrary. We should probably be inclined to consider a function of the first degree simpler than a function of the second degree; however, even the latter undoubtedly represents an unexceptionable law when it describes the data of observation with great accuracy. The Newtonian formula of gravitation, in which the square of the distance occurs, is generally still regarded as a paradigm case of a simple natural law. One may, for example, agree further that of all continuous curves that pass through a given number of points with sufficient proximity, we may consider as simplest the one that everywhere on the average has the greatest radius of curvature. (There is an unpublished work on this by Marcel Natkin.) However, such artifices seem unnatural, and the fact alone that there are *degrees* of simplicity makes the definition of causality based upon it unsatisfactory.

This state of affairs is made even worse by the fact that, as we know, it is not at all a matter of the simplicity of an isolated law, but of the simplicity of the system of all natural laws. And so, for instance, the true equation of the law of gases has by no means the simple form given to it by Boyle-Mariotte; yet we know that its complicated form may be explained by a particularly simple set of elementary laws. In principle it should be much more difficult to find rules for the simplicity of a *system* of formulas. They would always remain provisional, so that apparent order could with progressive knowledge turn out to be disorder.

And so neither the Maxwellian nor the aesthetic criterion seems to give a really satisfactory answer to the question as to what causality actually is. The first seems too narrow, the second too vague. No progress in principle is made by a combination of both attempts, and one may readily see that the shortcomings cannot be removed by improvements along the same lines. The shortcomings observed are clearly of a fundamental nature, and that gives us the idea of revising the present point of departure and of considering whether we are in general on the right path.

6 *Prediction as Criterion of Causality*

Until now we have assumed that a definite distribution of values is given, and asked: when does it represent a regular and when a chance sequence? It may be that this question cannot be answered at all by mere consideration of the distribution of values, but that it is necessary to go beyond this domain.

Let us for a moment consider the consequences that the statements made about the *concept* of causality have for the *principle* of causality. We imagine that for as many internal and boundary points of a physical system as possible we attempt to determine the value of the state variables by precise observation. Now one is in the habit of saying that the principle of causality is valid if from the state of the system during a very short time and from the boundary conditions all other states of the system may be deduced. Such a deduction is, however, possible *under all circumstances*, for according to what has been said, functions may always be found that represent all observed values with any desired accuracy. And as soon as we have such functions we may by means of them compute all states *already observed*, whether earlier or later, from any state of the system. For functions have been chosen in such a way that they represent everything observed in the system. In other words: the principle of causality would be satisfied *under all circumstances*. A statement, however, that is applicable to any system whatever, no matter what its properties, says nothing at all about this system, is an *empty* statement, a mere tautology, and it is futile to construct it. Hence if the causal law is actually to mean something, if it has content, the formulation from which we began must be false, for the law has turned out to be tautological. If, however, we make the qualification that the equations used are not to contain the space and time co-ordinates explicitly, or that they are to be very ' simple ', the principle acquires, to be sure, a real content; but, in the first case the reflection is valid that we have formulated too limited a concept of causality, and in the second the sole characteristic would be that the computation would be easier. However, we should certainly not want to formulate the difference between chaos and order in such manner that we say the first is comprehensible only to an excellent mathematician, the second to an average one.

We must therefore begin anew and attempt to formulate the meaning of the causal law differently. Our error until now was that we did not conform with enough precision to the actual procedure by which, in science, one actually tests whether processes are or are not dependent upon one another, whether or not a law, a causal sequence, exists. Until now we only investigated how a law is *constructed*. To learn its real meaning, however, one must observe how it is *tested*. It is always the case that the significance of a statement is revealed only by the manner of its verification. How then is the test made?

After we have succeeded in finding a function that satisfactorily connects a group of observational data, we are by no means satisfied, even when the function found has a very simple structure, for now comes the main thing, which our considerations hitherto have not touched: We observe whether the formula obtained also represents correctly those observations that *were not used* in achieving the formula. For the physicist, as an investigator of reality, the only important, decisive, and essential thing is that equations derived from certain data be applicable to other, *new* data. Only when this is true does he consider his formula to be a law. In other words, the true criterion of law, the essential sign of causality, is the *success of prediction.*

By success of prediction is to be understood, according to what has been said, nothing but the confirmation of a formula for such data as have not been used in its construction. Whether these data have already been observed or are only subsequently determined is in this connection of no consequence whatever. This observation is of great importance: past and future data are altogether on the same footing in this respect, the future is not of special significance; the criterion of causality is not confirmation in the future but confirmation in general.

It is self-evident that the test of a law can occur only *after* its formulation, but this gives no special distinction to the future. What is essential is that it is indifferent whether the verifying data are in the past or the future; it is incidental when they become known or are used for verification. The confirmation remains the same whether the data were known before the formulation of a theory, as in the case of the anomaly of Mercury's movement, or whether it was prophesied by the theory, as in the case of the red-shift of the spectral lines. Only for the *application* of science, for technique, is it of fundamental importance that natural laws allow prediction of something new, observed by no one as yet. And so earlier philosophers, Bacon, Hume, Comte, have long known that knowledge of reality coincides with the possibility of prediction. Thus fundamentally they correctly formulated the essence of causality.

7 *Elucidation of the Result*

If we accept the success of prediction as the true criterion of a causal relationship—and, with an important limitation to be mentioned presently, we shall have to do so—we thereby admit as well that the previous attempts at definition no longer enter into consideration. In fact, if we can really predict new observations, it does not matter how the formulas that enabled us to do so were constructed, whether they seem simple or complicated, whether time and space enter explicitly or not. As soon as someone can calculate the new observation data from the old, we shall admit that he has grasped the law governing the processes; prediction is therefore a sufficient characteristic of causality.

We easily realise that confirmation is also a necessary characteristic, and that the Maxwellian and aesthetic criteria do not suffice, when we imagine we have found a formula of great simplicity that describes a definite natural process with great precision but at once ceases to work when applied to the further phases of the process, to new observations. Obviously we should then say that the distribution of magnitudes occurring once has simulated a dependence of natural events which in reality does not exist, that it was a matter of mere chance the particular sequence could be described by simple formulas. That there was no natural law is proven by the fact that our formula can stand no test, for in the attempt to repeat the observations the sequence occurs quite differently; the formula is no longer applicable. A second alternative seems, of course, to be that one may say the law was valid at the time of the observation but no longer holds. It is clear, however, that this is only another way of expressing the absence of law, the universality of the law being denied. The 'regularity' observed for the single sequence was not true regularity, but merely chance. The confirmation of prediction is therefore the *only* criterion of causality. Through it alone does reality speak to us; the construction of laws and formulas is simply the work of man.

Here I must include two observations that go together and are of basic importance. First, I said previously that we may recognise the 'verification' of a regularity as the adequate characteristic of causality only subject to a limitation. This limitation consists in the fact that the confirmation of a prediction never actually *proves* the existence of causality but always only makes it *probable*. Further observations may indeed show the supposed law to be always incorrect, and then we should have to say that 'it expressed the sequence only by chance'. A final verification is therefore, so to say, impossible in principle. We deduce therefrom that a causal statement logically does not at all have the characteristic of a *proposition*, for a genuine proposition must in the end allow itself to be verified. We shall return to this shortly without, however, being able to explain this apparent paradox fully here, where we are not concerned with logic.

The second observation concerns the fact that between the criterion of confirmation and the two rejected attempts at definition a remarkable relationship exists nevertheless. It lies simply in the fact that *actually* the different characteristics go hand in hand. We certainly expect with great assurance that precisely those formulas satisfying the Maxwellian criterion and distinguished by aesthetic simplicity will be confirmed, and that the propositions made by their help will be true. And even if we should sometimes be disappointed in this expectation, the fact remains that the laws that have really proved to be valid were always of a profound simplicity, and always fulfilled the Maxwellian definition. But what the significance of this ' simplicity ' is, is difficult to say, and much erroneous thinking has been done in this connection; we do not wish to put too much stress upon it. It is certain that we may imagine much ' simpler ' worlds than our own. There is also a ' simplicity ' that is merely a matter of *representation*, that is, pertains to symbolism by means of which we express facts. Its consideration leads to the question of ' conventionalism ' and does not interest us in this connection.

At any rate we see that if a formula corresponds to both of the earlier and inadequate criteria we consider it probable that it is really the expression of a law, of an actually existing order, that it will therefore be *confirmed*. If it has been confirmed, we think it again probable that it will continue to be so. (And indeed, it is understood, without bringing in new hypotheses, for in general physical laws are so constructed that they may always be maintained by new hypotheses brought in *ad hoc*. But if these become too complicated, one says that the law nevertheless does not hold, the right order has not been found.) The word probability, which we use here, moreover designates something completely different from the concept treated in the calculus of probability and occurring in statistical physics.[1]

For the sake of logical clarity (for philosophers this is the prime concern) it is of greatest importance to realise the situation precisely. We have seen that, basically, causality is not at all definable in the sense that for an *already given* sequence one could answer the question: Was it causal or not? Only in reference to the *single case*, to the single verification, can one say: It behaves as causality demands. For advancement in knowledge of nature (and this is the main concern of the physicist) this is fortunately quite sufficient. If a few verifications—under some circumstances only one—are successful, we build practically on the verified law, with the unqualified reliance with which we trust our life to a motor constructed according to natural laws.

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[1] Cf. F. Waismann, ' Logische Analyse des Wahrscheinlichkeitsbegriffs ', *Erkenntnis* I, 238, with whose analysis I find myself in principle in full agreement.

It has indeed been frequently observed that one cannot actually speak of an absolute verification of a law, since we always, so to say, silently reserve the right to modify it on the basis of further experience. If I may in passing add a few words about the logical situation, the circumstance mentioned means that basically a natural law does not have the logical character of a 'proposition' but represents 'a direction for the formulation of propositions'. (I owe these ideas and terms to Ludwig Wittgenstein.) We have already indicated this, above, regarding causal statements, and actually a causal statement is identical with a law. The statement, 'The principle of energy holds', for instance, says no more nor less about nature than the principle of energy itself says. As is well known, only the individual propositions derived from a natural law are testable, and these always have the form: 'Under such and such circumstances this indicator will point to that mark on the scale', 'Under such and such circumstances there occurs a darkening on this point of the photographic plate', and the like. The verifiable propositions are of this nature and of this nature is every verification.

Verification in general, the success of a prediction, confirmation in experience, is therefore the criterion of causality, simply; and indeed in the practical sense in which alone we may speak of the test of a law. In this sense, however, the question regarding the existence of causality *is* testable. That confirmation in experience, the success of a prediction, is something *final*, not subject to further analysis, cannot be overemphasised. No number of propositions can state when it must occur, but we must simply await whether it occur or not.

8 *Causality and Quantum Theory*

In the previous considerations nothing was said except what, in my opinion, may be read out of the procedure of the scientist. No concept of causality was constructed; only the rôle it actually plays in physics was determined. Now the attitude of most physicists towards certain results of quantum theory shows that they see the essence of causality just where the foregoing considerations also found it, namely, in the possibility of *prediction*. When the physicists say that complete validity of the causal principle is not compatible with the quantum theory, the basis, indeed the *meaning*, of this assertion, lies simply in the fact that the theory makes precise *predictions* impossible. We must try to make this really clear to ourselves.

In contemporary physics it is possible to say, in a manner of speaking, that, with certain limitations to be mentioned, each physical system is to be considered as a system of protons and electrons, and that its state is completely determined by the position and momentum of its particles being known at every moment. Now, as is well known, a certain formula is derived from the quantum theory, the so-called ' uncertainty principle ' of Heisenberg, which teaches that it is impossible to indicate for a particle *both* determinants, place *and* velocity, with any desired precision, and that the more precise the value of one co-ordinate the greater inexactness we must expect for the other. If we know say that the place co-ordinate lies within a small interval Δp, the velocity co-ordinate q may be indicated only with such precision that its value remains undetermined in the interval Δq, and indeed so that the product $\Delta p \, \Delta q$ is of the order of magnitude of Planck's quantum effect h. In principle then the one co-ordinate could be determined with any degree of precision, but absolutely precise observation of it will have as a consequence that we can say *nothing more* about the other co-ordinate.

This principle of indeterminacy has been so frequently illustrated, even in popular form, that we need not describe the situation any more closely; our task must be to understand exactly its real *meaning*. When we ask for the meaning of a statement this always means (not only in physics): by which particular experiences do we test its truth? When thus, for example, we conceive the place of an electron to be determined by observation with an inexactness Δp, what does it mean when I say, for instance, the direction of the velocity of this electron may be indicated only with an inexactness $\Delta \theta$? How do I determine whether this statement is true or false?

Now, that a particle has gone in a definite direction may be tested only by its arrival at a definite point. To give the velocity of a particle *signifies* absolutely nothing more than to predict that in a certain time it will arrive at a certain point. ' The inexactness of direction amounts to $\Delta \theta$ ' means: in a certain experiment I shall find the electron within the angle $\Delta \theta$; however, I do not know exactly *where* therein. And if I repeat ' the same ' experiment I shall find the electron at various points within the angle, and I never know *beforehand* at which point in it. If the position of the particle is observed with absolute precision the result would be that in principle we could not know at all in which direction the electron would be found after a short time. Only further observation could subsequently tell us this, and with very frequent repetition of ' the same ' experiment it must appear that on the average no one direction predominates.

The fact that both position and velocity of an electron cannot be precisely measured is usually interpreted as saying: it would be impossible to describe fully the state of a system at a definite point in time, and therefore the principle of causality becomes inapplicable. Since the principle asserts that the future states of the system are determined by its initial state, since, thus, it presupposes that the initial state may be described in principle exactly, the principle of causality collapses, for this presupposition has not been fulfilled. I should not like to call this idea false, but it seems useless to me, because it does not express the essential point clearly. What is essential is that one realise that the indeterminacy that the Heisenberg-relation expresses is in truth an indeterminacy of *prediction*.

In principle nothing interferes (this is also emphasised by Eddington in a similar thought context) with our determining the position of an electron twice at any two closely adjacent points in time, and with our considering these measurements equivalent to position and velocity measurements. But the vital point is that with data about a state obtained in such a manner we are never in a position to predict a future state with precision. If, that is, we should define the velocity of the electron in the usual manner (distance divided by time) by means of the observed places and times, the velocity would nevertheless be different in the next moment, for, as we know, it must be assumed that its course is disturbed by observation in a quite uncontrollable manner. This alone is the true significance of the statement that a momentary state is not precisely determinable; that is, the impossibility of prediction alone is the actual reason why the physicist deems necessary the denial of causality.

There is no doubt, therefore, that quantum physics finds the criterion of causality precisely where we too have discovered it, and speaks of the failure of the principle of causality only because it has become impossible to make predictions with any desired degree of accuracy. I cite M. Born, *Naturwiss.* 17 (1929):

> The impossibility of measuring exactly all the data of a state prevents the predetermination of its further course of development. Because of this the principle of causality in its usual formulation loses all significance. For when it is impossible in principle to know all the conditions (causes) of an event, it is empty talk to say that every event has a cause.

Causality as such, the existence of laws, is however not denied. There are still valid predictions, but they do not consist in the expression of exact magnitude values, but are of the form: the magnitude X will lie in the interval between a and Δa.

What is new in the contribution of the most recent physics to the problem of causality does not consist in the fact that the validity of the causal principle is contested at all, nor that, say, the micro-structure of nature is described by statistical rather than causal regularities, nor in the fact that the realisation of the merely probable validity of natural laws has displaced belief in their absolute validity. All these ideas have, in part, long since been expressed. The novelty rather consists in the hitherto unsuspected discovery that through natural laws themselves a limit is set in principle to precision in predictions. That is something quite different from the rather obvious idea that actually and practically there is a limit to precision in observations and that the assumption of absolutely precise natural laws is in every case unnecessary if one wants to give an account of every experience. Previously it must always have seemed as if the question of determinism had to remain undecided in principle. The kind of decision now available, namely, by means of a natural law itself (the Heisenberg relation), was not foreseen. In any case, one who today speaks of a decidability and holds the question to be answered unfavourably for determinism must assume that law of nature as actually existing and raised beyond all doubt. That we are absolutely sure of this, or ever could be, a careful investigator will hesitate to state. But the principle of indeterminacy is an integral part of the structure of the quantum theory, and we must trust its correctness so long as new experiments and new observations do not force us to revise the quantum theory. (In fact it is daily better confirmed.) But to have shown that a theory of such structure is at all possible in the description of nature is in itself a great accomplishment of modern physics. It signifies an important philosophical clarification of the basic concepts of natural science. The progress in principle is clear. One may now speak of an empirical test of the principle of causality *in the same sense* as of the test of some special law of nature. And that we may in some sense justifiably speak of it is proven simply by the existence of science.

(To be concluded)

DISCOVERING THE POSITRON (I) *

NORWOOD RUSSELL HANSON

'On August 2, 1932, Dr Carl D. Anderson discovered the positive electron.'

THIS remark is often repeated in textbooks and manuals on modern physics. It is singled out here for several reasons: (1) Its innocent, matter-of-fact tone conceals one of the most intricate and interesting chapters in the history of scientific discovery; (2) it encourages an analogy between discoveries within microphysics, and quite non-comparable discoveries within, e.g., natural history; and (3) the actual discovery of the positron, in all its dimensions, constitutes an instructive example of the interplay of theory and experiment within physical science, one of the best a historian, or logician, of science could ever hope for.

The positron packet can be dipped into in three different ways. The physicist reaches into this complex of concepts to facilitate his thinking within e.g. quantum field theory, or the experimental study of cosmic rays. The historian of physics attends to the exciting interplay of ideas within the microphysics of the 1920s—the better to perceive the initial resistance to the positron-idea, as well as to trace the theoretical evolution which resulted in the claim that positive electrons exist. The logician will focus upon the internal structure of the arguments and concepts actually employed, and still being employed, by those physicists who have played major rôles in this story. In the history of science the positron discovery constitutes at once one of the richest, and yet one of the most neglected, areas of inquiry.

The discovery of the positive electron was a discovery of three different particles. I choose to put the matter this way, rather than to

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* Received 10. ix. 60. During the last six years this research has been helped by many. The Nuffield Foundation, The Minnesota Center for Philosophy of Science and Indiana University were especially encouraging. Moreover, the principals within this intricate drama of ideas are primarily responsible for whatever merit there may be in what follows. Professors Anderson, Dirac, Blackett, Oppenheimer, Mott, Bethe, Bohr, Heisenberg, Lamb, Skobeltzyn, Rosenfeld, Wilkinson, and Joliot-Curie helped with references, interpretations, and recollections. Professors Ryle, Feigl, Maxwell, Sellars, and Toulmin made valuable suggestions. I regret only that their efforts cannot guarantee that the sequel is free from errors and imperfections. These are, of course, solely the author's responsibility.

remark three different discoveries of the same particle, because the conceptual backgrounds within which the work of Dirac, Anderson, and Blackett took place were so disparate as to leave it unclear until almost 1934 whether their findings had anything in common. Perhaps this is not so dramatically apparent in the case of Anderson and Blackett, since the latter knew immediately that ' his ' particle was the same as Anderson's. Dirac's work is quite distinct, however.

Indeed, a contemporary reflection of this is found in the ways in which scientists now describe the discovery. Thus Millikan, writing in 1935, says ' . . . the discovery of the positive electron . . . was made without the guidance of any theory whatever, as was also the discovery of the frequent occurrence of positive-negative pairs of tracks . . . '.[1] Professor Blackett describes the discovery in much the same way, as do also Dr Oppenheimer and Anderson himself.[2] On the other hand, Professor Hans Bethe feels the discovery to have been primarily a theoretical one, on the grounds that positron tracks had been photographed before 1932. However, lacking a theory in virtue of which such tracks might be made intelligible, physicists failed to identify them. Indeed, Professor Konopinski avers that 'every theoretical physicist knows that Dirac discovered the positron '. Thus, even today, these historically different approaches to the particle remain unresolved.

Accordingly, our study will be designed as a triptych; we shall discuss (1) The Anderson Particle, (2) The Dirac Particle, and (3) The Blackett-Occhialini Particle.

Even before considering Anderson's research, some account of the genesis of the present essay may be in order. Professor P. A. M. Dirac once spoke to me of a lecture given at the Cavendish by D. Skobeltzyn, ' sometime in 1926 or 1927 '. Dirac recalls the description by someone, perhaps Skobeltzyn, perhaps someone in the audience, of an experimental setup within which Skobeltzyn was bombarding a metal target. One of the curiosities reportedly mentioned by Skobeltzyn was that several particles which were certainly electrons were seen to ' fall back into the source'; this, despite the fact that most of the electrons moved in the way usual for this experiment, *away* from the source. Professor Dirac feels that what he remembers Skobeltzyn as having then described could only have been positive electrons, and he suggests that the Russian might very well then have made the discovery.

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[1] R. A. Millikan, *Electron* (Cambridge, 1935), p. 339. Compare Neddermeyer, Anderson's collaborator in 1932 and 1937, writing in the *Physical Review*, March 15, 1961 : ' However, it is not to discredit theory or theoreticians to point out that the muon, like the positron, was a purely experimental discovery in the sense that it was made entirely independently of any theoretical considerations of what particles should and should not exist. . . .' (p. 1814).

[2] These sentiments were disclosed to the author during conversations and correspondence with the physicists mentioned.

My reaction was that it would have been an immensely difficult feat of imagination for Skobeltzyn, or anyone else, to have made this discovery in 1926, given the theoretical conceptions and experimental facilities then available. An exploration of Skobeltzyn's published work entirely failed to reveal any photograph or even a passing remark, relevant to Dirac's narrative. However, a related photograph did turn up in *Zeitschrift für Physik*, 1927, **43**, 362. Schematically-reproduced below is part of the lower left hand photograph on that page:

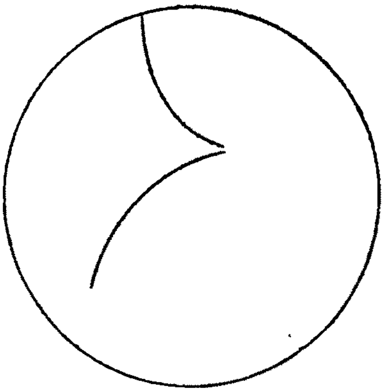

Fig. 1.

Let me stress that this configuration is probably not what it appears to be. None the less, the presence of geometrically-associated tracks, of the sort one expects with positron-negatron pairs, will be evident. As a check on my own reactions this photograph was shown to many experimentalists. Without exception, all granted that this might well be a pair; some thought it a very good example of one. No stereoscope was used in this survey, however.

Professor Skobeltzyn, in two communications of 1956 and 1960, offers a different interpretation of this photograph. He takes it to be a chance overlapping of two different tracks, one of a Compton recoil-electron, and the other of a spurious track emerging from the chamber wall. Without doubt, Skobeltzyn is correct, but since the original plates are destroyed, the use of a Pulfrich Stereocomparator is not now possible. The foregoing plate is to be understood more as a trigger for my further inquiries concerning pre-Anderson positrons, than, as had until recently been felt, an actual disclosure of such a particle. For, whether justified by the event or not, the present author was encouraged by this photograph to seek further pre-1930 evidence of positive electrons. It can be confidently supposed that the tell-tale tracks were 'encountered' by researchers like Orban, Rochester, Terroux, Feather, and certainly by Meitner, and the Joliot-Curies. Indeed, in pointing up the mistake in Dirac's reports and recollections concerning 1926-27, Skobeltzyn refers me to the work of Joliot and Curie of early 1932 in which mention is made of 'backward electrons '.[1]

Skobeltzyn suggests that Dirac may have confused this work of Joliot and Curie with his memory of the 1926-27 lecture at the Cavendish. Perhaps. But Blackett, Meitner, and others certainly encountered electrons, before Anderson's discovery, which they described as 'falling back into the source', 'curving the wrong way', 'coming up from the floor', or 'moving backwards'. Skobeltzyn disagrees with my contention that several microphysicists saw, but did not observe, positron tracks prior to 1930. He writes:

> None of them published their results earlier than in 1930. Besides, Williams and Terroux used in their work RaE, a source which does not emit γ-rays. Meitner-Filipp published their observation in 1933 and certainly did not use the Wilson chamber combined with a magnetic field until 1932. Ellis never worked with a Wilson chamber. Joliot-Curie began the observation with a Wilson chamber placed in a magnetic field only in 1932 (certainly not earlier than the end of 1931, i.e., definitely after my departure from Paris in August 1931). D. S. [Letter to the author, 22nd October 1960.]

Professor Skobeltzyn's views must, of course, be given the greatest weight. Against this, however, must be balanced the reports of Dirac, Blackett, and Bethe, all of whom have expressed to me their conviction that tracks were encountered, but not identified, long before Anderson's discovery. Since this essay attempts to provide an historical and conceptual context for this reported conviction, I have placed Skobeltzyn's views alongside those of other physicists to give the reader the same data that the author has.

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[1] '. . . il nous a paru que le parcours des rayons H projetés par les neutrons émis vers l'arrière, par rapport à la direction des particules α, est plus grand que celui qu'on peut prévoir par le calcul . . . Un rayonnement très pénétrant s'absorbant par projection de noyaux et émis dissymétriquement par rapport à la direction des particules α incidents . . .'. (I. Curie and F. Joliot, *Comptes Rendues de l'Académie des Sciences*, **194**, 1232; 11th April 1932.)

In this connection, the following is relevant:

> I heard about 'electrons falling back into the source' at a colloquium at the Cavendish. . . . Someone was reporting on the work of Skobeltzyn, the Russian physicist . . . I would suggest you look up *Science Abstracts* for the years 1925-30 . . . [Dirac, letter to the author, 12th August 1955.]
>
> . . . all sorts of people saw positrons well before Anderson. Blackett certainly did and was always puzzled by the number of electrons that came upwards from the floor . . . [D. H. Wilkinson, letter to the author, 12th December 1955.]
>
> . . . Nobody took 'experimental' positive electrons before 1933 seriously, nor did Dirac make the connection in 1931. The theoretical development was certainly taken much more seriously . . . [H. Bethe, communication to the author, early 1956.]

Whenever seen, such tracks were discounted as 'spurious', or as 'dirt effects'. Certainly, no experimental physicist before late 1932 made any such track his prime object of study. Part of the function of our inquiry will be to understand why this is so, why such tracks were always overlooked, underevaluated, or explained away. Why did the idea of a positively-charged electron seem so completely untenable?

1 *The Anderson Particle*

Had anyone ever seriously suggested that Professor Anderson's discovery of the positive electron was contingent upon the prior publication of Dirac's positron theory in 1931 [I once suggested this], Anderson could easily refute him. Neither Dirac's book *Quantum Mechanics* (1930), nor his great paper on the spinning electron (1928)[1] had even been read by Anderson before 1932. Indeed, he claims not to have fully understood either of these works even when he read them later, having been encouraged to do so by the publication of Blackett-Occhialini in 1933. Hence it could hardly be claimed that Anderson's discovery of the positron on 2nd August 1932, was the result of Dirac's theoretical breakthrough. No, Anderson's discovery was of a different kind.[2]

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[1] *The Principles of Quantum Mechanics*, Oxford, 1930; 'The Quantum Theory of the Electron', *Proc. Roy. Soc.*, 1928, **117**, 610; **118**, 351

[2] Dr Oppenheimer writes: '. . . I remember talking with Anderson about the possible relevance of his tracks and the Dirac theory. His attitude was that he did not understand the theory, and could not take a responsible view of it: and that therefore his discovery had to be soundly based on experimental material . . .' [Letter to the author, 12th August 1960.]

The celebrated photograph sketched below is that for which Carl Anderson is honoured. But the honours accrue not simply to his camera technique and experimental ingenuity.[1] For here was a cloud chamber track which might also have been rejected as ' spurious ', or even as a ' dirt effect '. Soon we will examine the reasons for which Anderson's predecessors and contemporaries were so reluctant to admit a positive electron. Theory was against it. Observation was against it. The giants of physics, Bohr and Rutherford, were against it. But Anderson had the courage, and the cool logic, to be for it. Although he knew of no established theoretical route along which to infer the existence of a positron electron, Anderson's honest and rigorous interpretation of the photograph schematically-reproduced above forced him to hack out his own argument *ab initio*:

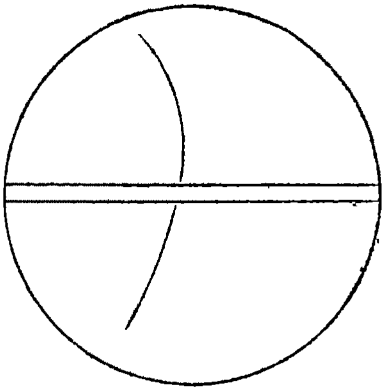

Fig. 2.

> . . . the track . . . cannot possibly have a mass . . . of a proton . . . [. . . the length . . . is . . . ten times greater than . . . a proton path of this curvature] . . . assume that at exactly the same instant . . . two independent electrons happened to produce two tracks . . . to give the impression of a single particle. . . . This . . . was dismissed on a probability basis . . . we also discarded . . . the assumption of an electron of 20 million volts entering the lead on one side and coming out with . . . 60 million volts. Other photographs were obtained which could be interpreted logically only on a positive-electron basis . . . (op. cit. p. 491).

In short, Anderson reasoned that the particle was coming up from below: it lost energy in the plate and curved more markedly towards the negative pole of the transverse magnetic field surrounding the chamber. This established that the particle, already identified as an electron by its range, was positively-charged.

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[1] C. D. Anderson, ' The Positive Electron ', *Physical Review*, 1933, 43, 492

In his distinguished cosmic ray work of early 1931 Professor R. A. Millikan had already obtained two remarkable photographs, sketched below.[1] These record incoming cosmic rays encountering an atomic nucleus, out of which are projected a positive and a negative particle.

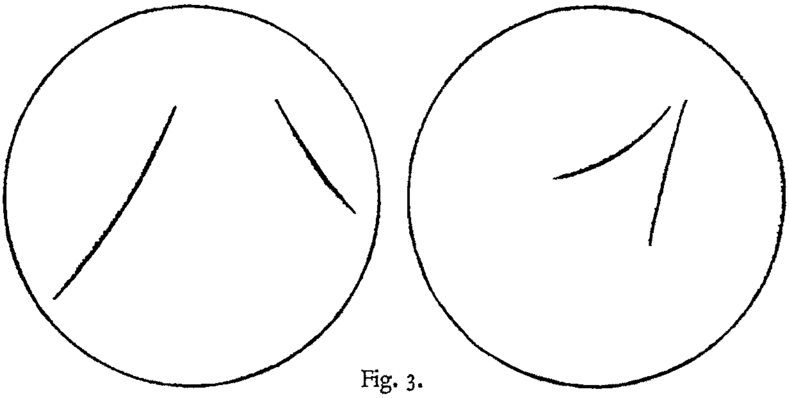

Fig. 3.

Millikan's photographs established that the nucleus is important in cosmic-ray absorption; moreover, both positives and negatives can be ejected from a nucleus hit by a cosmic ray. In 1919 Rutherford had shown that α-particles could dislodge protons from the nuclei of light atoms. This was the nearest analogy to Millikan's observations. But cosmic rays are not streams of α-particles. So here was a new phenomenon.

In 1931, during lectures at Cambridge and Paris, Millikan interpreted the positive components of these double tracks as protons. The photographs were published on 18th December 1931.[2] This interpretation did not work with the second of the two photographs. The positive particle had little curvature; Millikan was forced to conclude that it was a proton of 450 mev, while the negative track was taken as an electron of 27 mev. At this energy the ionisation of a proton should be between 1·5 and 2 times that of an electron of 27 mev. But there was no detectable difference in ionisation between the positive and negative particles. This fact runs counter to certain well-established correlations.[3] Millikan thought there was something wrong with the theory of how the ionisation of protons varies with energy. He did

[1] Cf. *Electrons*, Chapter XIV, Figs. 54 and 55

[2] 'Cosmic Rays Disrupt Atomic Hearts' in *Science Service*

[3] Williams and Terroux, *Proc. Roy. Soc.*, 1929–30, **126**, 300

not then, nor did Anderson, ever suppose that the positive particle might be other than a proton. Millikan and Anderson agreed to come back to this 'proton' to discover the 'error'. But even now they knew that since an isolated single track of small ionising power curved rapidly to the right, and hence ought to be a positive particle going down, the track might also be considered as having been made by a negative going up. It was necessary to have two tracks branching downward from a common centre for any positive identification of a positive as opposed to a negative particle.[1] Only a small percentage of all the particles photographed by Anderson and Millikan were so 'associated'. Therefore, the 'proton' interpretation of the second photograph was difficult to check. Moreover, for reasons to emerge, the assumption that the fundamental unit of positive electricity *could only* be associated with the proton, made active challenging of the Millikan-Anderson interpretation seem *prima facie* implausible.[2]

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[1] Cf. Skobeltzyn, *Zeitschrift für Physik*, 1929, **54**, 686; this is the first known to such branch tracks.

[2] Concerning this work of Millikan and Anderson, Professor Skobeltzyn has kindly sent me correspondence of the period from L. H. Gray, M. Curie, and F. Joliot-Curie. The relevant excerpts are set out here with the permission of the authors or their heirs or executors.

Cambridge, Nov. 27th 1931

Dear M. Skobeltzyn,

On Monday we had a visit from Millikan . . . he showed eleven photographs of 'good' tracks . . . which from the sense of their curvature must have been positive particles . . . In some cases a proton (?) and electron appeared on the same photograph, and with the help of a vivid imagination one *might* conclude that they had a common origin . . . in every case the proton tracks were more dense than those of the electrons. [This was not in fact true. But that Gray should have expressed himself thus is significant for our thesis.—N. R. H.] Their curvature was clearly visible in some cases, though not of course large . . . the general feeling was that the curvatures could hardly have been spurious . . . A large proportion of double tracks, as you had previously found, is in itself very significant . . .

Yours sincerely
L. Harold Gray

Paris, le 21 Décembre 1931

Cher Monsieur,

. . . Le point le plus important est que sur ces clichés [of Professor Millikan] on voit, non seulement des électrons, mais aussi des protons (déviation en sens inverse), et qu'on a vu, en particulier, des trajectoires d'électron et de proton paraissant partir d'un même point. Les potentiels indiqués par les courbures seraient de l'ordre de 100 millions de volts . . .

Veuillez agréer, Cher Monsieur, l'assurance de mes meilleurs sentiments.

M. Curie

Then came the photograph of 2nd August (cf. above). After encountering some further unambiguous cases Dr Anderson published his discovery.[1]

Nonetheless, the appearance of 'free' positrons continued to be regarded as a rare and peculiar event, even by Anderson. Indeed, it is reliably reported that when Bohr and Rutherford heard of Anderson's publication, they were full of scepticism. Prior theoretical commitments prepared them to feel *a priori* that a wrong interpretation must have been given to whatever it was that Anderson photographed. So most high energy positive tracks encountered during subsequent months were interpreted as protons. During all this rigorous questioning and reinterpretation, the best photograph remained the original one, that of 2nd August 1932.

In early 1933 Anderson published his most notable report, 'The Positive Electron'.[2] There, besides reproducing the original photograph, and setting out his hard reasoning in favour of a positron—reasoning quoted earlier—Anderson set out also a photograph appearing as follows:

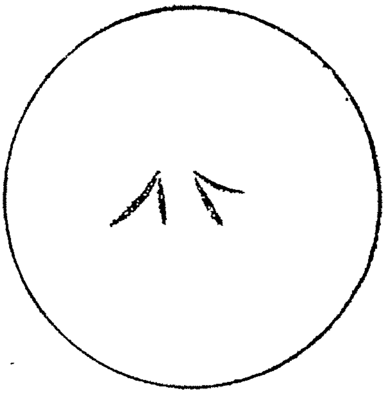

Fig. 4.

Mon cher Skobeltzyne, Paris, le 1er Janvier 1932

. . . ses collaborateurs [i.e., Millikan's] ont photographiés des trajets de rayons cosmiques dans un champs magnétique élevé (13,000 G) et ils ont vu que certaines trajectoires sont dues à des électrons et d'autres à des protons. Il a même projeté un cliché d'où l' on voit issu du même point un électron et un proton d'énergie considérable 50 × 10^6 ev environs . . .

F. Joliot-Curie

And Professor Skobeltzyn himself remarks that as early as 1931 he 'prior to others, observed electron-positron pairs, not being able to identify them, however. (My note in "Nature" v. 133, p. 23, 1934.)' [Letter to the author, 22nd October, 1960.]

[1] In *Science*, September 1932, **76**, 238

[2] C. D. Anderson, *Physical Review*, 1933, **43**, 491; cf. also 1034

The outside (far left and far right) tracks are 'associated in time'. That is, they represent ionisations which certainly occurred at the same instant; they are all diffuse to the same degree.[1] The track at the extreme left is of a negative electron of 18 mev. That at the extreme right is of a positive electron of 20 mev. The diffuseness of both tracks results from the cloud chamber expansion having occurred $\frac{1}{8}$ second after the passage of the ionising particles. The ions have thus diffused away from the original track before being stopped by water vapour condensation. Hence, under a microscope, one can accurately count the ions per centimetre of path, as well as measure the curvature. These two considerations determined both the charges and masses of the two particles as exactly the same to within 10 per cent and 20 per cent respectively.[2] Within these limits positive and negative electrons now seemed 'identical twins', the only difference being the sign of their charge. Indeed, in September of 1933, Anderson writes:

> The striking fact that in the cosmic rays positives and negatives occur in practically equal numbers and have apparently a similar energy distribution is in accord with the assumption that the nuclear effects involved give rise to the positives and negatives in pairs (in some cases several pairs as evidenced by the showers) . . . *Phys. Rev.* 1933, **44,** 416.

This, then, constitutes part of the chronology of the discovery of the Anderson positron. Fully to appreciate the concentration, and the challenge to theoretical orthodoxy which Anderson's interpretation of his data required—this will only begin fully to come out as one explores the contexts of two further discoveries. These were the discovery of the Dirac particle, and the Blackett-Occhialini particle.

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[1] These tracks are geometrically associated as well, and hence necessarily time-associated. Given the relative infrequency of cosmic-ray tracks in 1931 (1 in 30 exposures) the likelihood of independent events originating from a common centre is vanishingly small.

Compare Anderson, writing in 1933: 'The tracks . . . were coincident in time as shown by the fact that the diffusion of the ions broadened all the tracks to the same extent . . . Seven of the tracks were seen to originate at a common point in the upper portion of the chamber . . . The sense of curvature . . . is such as to indicate electrons . . . Protons . . . of the degree of curvature observed would have energies too low to be consistent with the observed minimum ranges and specific ionization . . . Obviously electrons cannot be the immediate ionizing agents . . . since more than 1000 electrons would have to be assumed to traverse the ion-chamber simultaneously . . .' (*Phys. Rev.*, 1933, **43,** 369).

[2] Cf. ibid., 1933, **44,** 406

2 *The Dirac Particle*

The new quantum mechanics of Schrödinger and Heisenberg was not in agreement with experiment during late 1927 and early 1928. When applied to the atom with point-charge electrons, it failed to account for duplexity phenomena, and the empirically discovered spin-angular-momentum ($\frac{1}{2}$ quantum).[1]

In one of the greatest theoretical papers in the history of physics, Dirac begins by saying:

> One would like to find some incompleteness in the previous methods of applying quantum mechanics to the point-charge electron such that, when removed, the whole of the duplexity phenomena follow without arbitrary assumptions . . .

He goes on:

> . . . the incompleteness of the previous theories [lies] in their disagreement with relativity, or alternatively, with the general transformation theory of quantum mechanics.[2]

In a most effective contribution to the new quantum mechanics Gordon assumes that the quantum-theoretic operator of the wave equation should be got from the classical relativity Hamiltonian by putting into F, $W = ih\,(\delta/\delta t)$, $pr = -ih\,(\delta/\delta x_r)$, $[r = 1, 2, 3]$.

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[1] This paragraph is virtually a paraphrase of Dirac's prefatory remarks in the 1928 paper alluded to in the next footnote. When Professor Heisenberg read this paragraph, however, he reacted as follows: '. . . I cannot quite agree with the first four lines of Chapter II. . . . Here your report neglects completely the fact that at this time the theory of the electronic spin had been well-established. The multiplet structure of the atomic spectra did not at that time offer any essential difficulty for quantum mechanics. Both the multiplet structure and the anomalous Zeeman effect were well understood, and Pauli had introduced the Pauli spin matrices with the well known properties: $\mathscr{C}_x\mathscr{C}_y = -\mathscr{C}_y\mathscr{C}_x = i\mathscr{C}_z$, etc. I cannot doubt that Dirac had been led to his discovery by Pauli's paper and especially by the relation $(p_x\mathscr{C}_x - p_y\mathscr{C}_y - p_z\mathscr{C}_z)^2 = p^2$. The essential progress in Dirac's paper was the connexion of Pauli's spin matrices with the Lorentz group. Dirac succeeded in representing the complete Lorentz group by introducing besides the Pauli spin matrices another similar matrix ζ which in some way represents the space reflections in the Lorentz group. It is the introduction of this ζ matrix which finally led to the dualism between matter and antimatter . . .' [Letter to the author, 19th September 1960].

[2] P. A. M. Dirac, 'The Quantum Theory of the Electron', *Proc. Roy. Soc.*, 1928, **117**, 610. Cf. also Pauli, *Zeitschrift für Physik*, 1927, **43**, 601; and Darwin, *Proc. Roy. Soc.*, 1927, **116**, 227.

This gives

$$F\psi = [(ih\,(\delta/c\delta t) + e/c\,(A_o))^2 + \Sigma_r\,(-\,ih(\delta/\delta x_r) + \\ + e/c\,(A_r))^2 + m^2c^2]\,\psi = 0 \qquad (1)$$

ψ being a function of X_1, X, X_3, t.[1] Gordon's equation was valuable and influential. But it involved two difficulties which Dirac remarks with clarity. The first concerns the physical interpretation of ψ. Gordon and Klein[2] made the aforementioned assumption, and it succeeded with the emission and absorption of radiation. But it was not general enough to answer the question 'What is the probability of a dynamical variable at t having a value lying between specified limits, when the system is represented by a given wave function ψ_n?'

> . . . the Gordon-Klein interpretation can answer such questions if they refer to the position of the electron . . . but not if they refer to its momentum, or angular momentum, or any other dynamical variable. We should expect the interpretation of the relativity theory to be just as general as that of a non-relativity theory. (Dirac, ibid. p. 612.)

The crux is this: in non-relativity quantum mechanics the wave equation is linear (of the first order in time) 'so that the wave function at any time determines the wave function at any later time. The wave equation of the relativity theory must also be linear in W if the general interpretation is to be possible' (ibid.). In short, Gordon's equation was not general enough: it could not give values to the dynamical variables when the system is represented by ψ_n.

The 1926 equation was of the second order in time (i.e. in $\delta/\delta t$). It is, of course, mathematically impossible to apply the general theory of transformations to differential equations higher than the first order in time.

So the first of the two difficulties Dirac perceives in the earlier Gordon equation consists in its not being of the first order in $\delta/\delta t$. He cannot be satisfied with such an equation.

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[1] Although written precisely as it appears on page 611 of Dirac's 1928 paper, scrutiny reveals that this equation (the Klein-Gordon) is improperly written. Dirac should have put a minus sign after the first square bracket, giving an equation of the form: $F = [-(W/c + e/c\,A)^2 + (p + e/c\,A)^2 + m^2c^2]\,\psi = O$. As they stand, equations (1) and (3) in Dirac's paper are formally inconsistent, a fact unnoticed during these thirty-three years. Professor Dirac has confirmed the present author's suggested emendation of (1), and of the equation in the middle of page 612. The latter appears unaltered in the quotation below, which begins 'The second difficulty . . .'. Again, a minus sign after the square bracket is necessary.

[2] Gordon and Klein, *Zeitschrift für Physik*, 41

> The second difficulty in Gordon's interpretation arises from the fact that if one takes the conjugate imaginary of [(1)] one gets
>
> $$[(-(W/c)+(e/c)A_o)^2+(-p+(e/c)A)^2+m^2c^2]\psi=0,$$
>
> which is the same as one would get if one put $-e$ for e. The wave equation (ibid. p. 611) thus refers equally well to an electron with charge e as to one with charge $-e$. If one considers for definiteness the limiting case of large quantum numbers, one would find that some of the solutions of the wave equation are wave packets moving in the way a particle of charge $-e$ would move on the classical theory, while others are wave packets moving in the way a particle of charge e would move classically. For this second class of solutions W has a negative value.[1] One gets over the difficulty on the classical theory by arbitrarily excluding those solutions that have a negative W. One cannot do this on the quantum theory, since in general a perturbation will cause transitions from states with W positive to states with W negative. Such a transition would appear experimentally as the electron suddenly changing its charge from $-e$ to e, a phenomenon which has not been observed. The relativity equation should thus be such that its solutions split up into two non-combining sets, referring respectively to the charge $-e$ and the charge e. (Ibid. p. 612.)

In short, the otherwise effective Gordon equation, besides being of the second order in time, also generated solutions involving particles with ' negative energy '. Such electrons had not been observed, or at least not identified, by 1928.

> In the present paper we shall be concerned only with the removal of the first of these two difficulties.

This last remark is historically interesting. Positron transitions are often referred to in the literature of theoretical physics as ' Dirac jumps '. But, considering the occasion for Dirac's problem, and his specific attack, it would be more appropriate to call such transitions ' Gordon jumps '. The original ' negative energy solutions ' were in print in 1926.[2] The problem was one of two which arose within the new quantum mechanics; it was the problem Dirac chose *not* to deal with in 1928. His problem was quite different. It was to ' obtain a wave equation of the form $(H-W)\psi=0$ which shall be invariant under a Lorentz transformation and shall be equivalent to [(1)] in the limit of large quantum numbers . . .' (ibid. p. 613).

The awe with which Dirac's 1928 paper was greeted by physicists was a function of the successful solution he found for this one problem.

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[1] Author's note: that is, these solutions have a ' negative energy '.

[2] Cf. Gordon, *Zeitschrift für Physik*, 1926, 40, 117 ff.

In an elegantly powerful demonstration, he designs a new wave equation $(i\Sigma\gamma_\mu p'_\mu + mc)\psi = 0$, which is not only of the first order; it can also be brought back into the form of the original Gordon equation, so that the important results of the latter stand out independent of the frame of reference used.'[1] Dirac immediately proves the new equation sufficient to account for all duplexity phenomena; he works the spin-angular-momentum ($\frac{1}{2}$ quantum) into the logical structure of his new equation, instead of being, what it had been, a merely empirical adjustment, theoretically *ad hoc* and unconnected with general electron theory.[2] Beyond all this, Dirac's equation gave all the correct results for the Hydrogen atom; the Zeeman effect became calculable and explicable; the fine-structure formula comes out as a consequence; and the entire Dirac electron theory was applied [by Klein and Nischina] to the Compton scattering of electrons.

Theoretical physics had not witnessed such a powerful unification of concepts, data, theories, and intuitions since Isaac Newton proposed his Principle of Universal Gravitation. From a chaos of apparently unrelated facts and ideas, Newton in his way, and now Dirac in his, built a logically powerful and conceptually beautiful physical theory.

For this reason Dirac's paper is regarded by theoretical physicists as 'the greatest paper of our century'.[3] However, the second difficulty within the Gordon equation, and hence the Dirac theory, remained. Dirac felt this to be a serious blemish.

In the same way, and for the same reason, Dirac sought to deal with this perplexity, as Anderson and Millikan had originally tried to deal

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[1] The Dirac equation for an electron moving in an arbitrary electromagnetic field of potentials A_0, A_1 A_2, A_3, is thus of the form:

$$F\psi = [p_o + (e/c)\,A_o + \alpha_1(p_1 + (e/c)A_1 + \alpha_2(p_2 + (e/c)A_2) + \\ + \alpha_3(p_3 + (e/c)A_3 + \alpha_4 mc]\,\psi = 0 \qquad (2)$$

[2] This in no way minimises the great achievement of Goudsmit and Uhlenbeck (1925). Their brilliant hypothesis, however, was intended only to 'save the phenomenon', and not to strengthen electron theory. Cf. *Nature*, 1926, **117**, 264. Cf. Dirac: 'The αs [in (2)] . . . may be regarded as describing some internal motion of the electron, which for most purposes may be taken to be the spin of the electron postulated in previous theories . . .' (*Proc. Roy. Soc.*, **118**, 351) . . . '. . . We can interpret this result by saying that the electron has a spin angular momentum of $\frac{1}{2}hp$, which, added to the orbital angular momentum m, gives the total angular momentum M, which is a constant of the motion . . .' (*Proc. Roy. Soc.*, **117**, 620). And please compare Heisenberg's remarks on spin, introduced in the first note in this chapter. Cf. also 'Exclusion Principle and Spin' by B. L. Van der Waerden, in *Theoretical Physics in the Twentieth Century*, New York, 1960, especially pp. 232-233.

[3] This is Professor Hans Bethe's remark.

with theirs.[1] Dirac, following a suggestion of Weyl, tried to ' cook ' the negative-energy solutions so that they might be construed as protons.[2] Had he been successful, his paper might have seemed even greater, in 1928. For then, it would have been the complete theory of elementary particles ; protons and electrons were then thought to be ' all there were '.

But then Weyl[3] demonstrated this interpretation to be untenable. Whatever might be the further properties of the ' negative energy ' particles, they had to have the same mass as the negative electrons. This was built into the logic of the Dirac paper, and this could not be tinkered with without wrecking the symmetry of the entire argument. So any appeal to protons was now blocked.

At this time, Gamow, in his serio-comic manner, dubbed the new particles ' donkey electrons '. Taking them to be electronic in every other way, Gamow simply accepted that they had negative energy. Like donkeys, the harder you pushed them away, the more forcefully they came at you. Hence, a direct hit with a γ-ray photon, should send the donkey electron ' recoiling ' back into the source with high velocity. Thus Gamow's suggestion!

But this hypothesis, and the negative energy theory in general, was physically unintelligible. It hardly helped the experimenter who had just witnessed an electron falling back into the source to be told that it had negative energy.

Accordingly, Schrödinger sought to modify the Dirac equation.[4] He aimed to restructure the algebra so as to block such awkward consequences, but not in so transparently *ad hoc* a way as to constitute a mere rejection. Although at first promising, the results were shown, I think by Oppenheimer, not to be Lorentz-invariant. It is philosophically interesting to realise that this lack of relativistic invariance was, by itself, enough to mark down the Schrödinger modification as a failure. It was more important to theoreticians to have an equation which, although it harboured unintelligible solutions, was none the less Lorentz-invariant, than to adopt an alternative equation which, although easily interpretable physically, lacked in variance. The requirements of ' operationalism ' alone are sometimes not quite enough to force a change in a broad-based theory.

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[1] At first Dirac simply excluded these solutions, *ad hoc*: ' Since half the solutions must be rejected as referring to the charge $+ e$ on the electron, the correct number will be left to account for duplexity phenomena . . . ' op. cit. p. 618. And even in 1930 Oppenheimer writes: ' . . . since the Dirac jumps do not seem to be *directly* responsible for the difficulties with which we are, in this work, most concerned, we shall for the present neglect them ' (*Phys. Rev*,. 1930, **35**, 461).

[2] In this connection Dirac has written to me: ' The world as we see it is not symmetrical between positive and negative electricity, so a theory that makes it symmetrical would seem to be at fault. That is why I tried to cook the negative energy solutions. The lack of symmetry is still not adequately explained.' (Letter to the author, 1st November 1960.)

[3] Weyl, *Gruppentheorie und Quantenmechanik*, 2nd edn., p. 234

[4] Schrödinger, *Berl. Ber.* (1931), S. 63

Now the negative-energy solutions began to worry Dirac in earnest. They could not be rejected, nor written out of the theory, à la Schrödinger; the very design of an otherwise successful equation would collapse by either such attempt. Nor could they merely be christened ' donkey electrons ', possessed of ' negative energy '; this solves nothing since the idea is unsound. Nor could these solutions be ' cooked ' into protons, as Dirac once hoped; Weyl showed that. So these perplexing solutions could no longer be lived with comfortably.

In 1930 Oppenheimer echoes the march of Dirac's thought.

> . . . Dirac has suggested [*Proc. Roy. Soc.*, 1930, **126**] that the reasons why the transitions of an electron to states of negative energy, which are predicted by his theory of the electron, do not in fact occur is that nearly all the states of negative energy are already occupied. Dirac has further shown that the unoccupied states of negative energy have many of the properties of protons . . . they may be represented by wave functions which would be taken to correspond to a particle of positive charge and positive mass . . . the mass associated with these gaps is not necessarily the same as that of the electron, and he has suggested the assumption that the gaps are protons. . . . According to Dirac the scattering takes place by a double electron jump, in which a negative electron [i.e. a negative-energy electron] jumps up into some state of positive energy and the original positive [i.e. ordinary] electron falls down into the gap left. There are several grave difficulties which arise when one tries to maintain the suggestion that the protons are the gaps of negative energy, and that there are no distinctive particles of positive charge. . . .[1]

Oppenheimer here refers to Dirac's ' proton-cooking ' phase; he also is on the verge of indicating, what Weyl was also concerned to do, that this move cannot work.[2]

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[1] Oppenheimer, ' On the Theory of Electrons and Protons ' *Phys. Rev.*, 1930, **35**, 562

[2] The main reasons why this won't work are that (1) If it did one would have to give an infinite density to positive electricity, (2) the scattering of soft light by a

> If we return to the assumption of two independent elementary particles, of opposite charge and dissimilar mass, we can resolve all the difficulties raised in this note, and retain the hypothesis that the reason why no transitions to states of negative energy occur, either for electrons or for protons, is that all such states are filled. In this way, we may accept Dirac's reconciliation of the absence of these transitions with the validity of the scattering formulae.[1]

Slightly earlier, Oppenheimer had expressed his attitude toward the 'Dirac jumps'.

> On the present theory there is no normal state for matter, because states of infinite negative energy are possible; one may in fact show that, on the present theory Dirac jumps to such states from states of positive energy—jumps in which the energy and momentum lost by the matter are taken up by the field—are not only possible, but infinitely probable. But that the theory should have predicted this is a token of an error in the theory. . . .[2]

It is to this 'error in the theory' that Oppenheimer, in the previous quotation, addressed himself.

The Dirac conjecture responsible for Oppenheimer's reactions can now be set out:

> The most stable states for an electron (i.e. the states of lowest energy) are those with negative energy and very high velocity. All the electrons in the world will tend to fall into these states with emission of radiation. The Pauli exclusion principle, however, will come into play and prevent more than one electron going into any one state. Let us assume that there are so many electrons in the world that all the

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proton is unaccountable-for on the Dirac theory; '. . . [the Dirac] theory gives equal scattering coefficients for electron and proton . . . such interaction would affect electron scattering and proton scattering in exactly the same way; whereas the Thompson formula requires the latter to be smaller by a factor proportional to the square of the ratio of the masses', and (3) there is a numerical discrepancy in the original Dirac supposition; a mean life for ordinary matter of the order of 10^{-10} seconds. 'We should hardly expect any state of negative energy to remain empty' (ibid. p. 563).

Cf. also Oppenheimer, *Phys. Rev.*, 1930, **35**, 939: '. . . we compute the rate at which electrons and protons should, on Dirac's theory of electrons and protons, annihilate each other'; . . . 'The mean lifetime of an electron in a proton density N_p proteins per unit volume is thus $T = (m^2/c^8/16\pi 5 e^4 n_p) \sim 5 \times 10^{10}/n_p$ sec . . . an absurdly short mean lifetime for matter' (p. 943). Cf. also Dirac, *Proc. Roy. Soc.* 1931, **133**, 61.

[1] loc. cit.

[2] Oppenheimer, 'Interaction of Field and Matter', *Phys. Rev.*, 1930, **35**, 461

> most stable states are occupied, or more accurately, that all the states of negative energy are occupied except perhaps a few of small velocity. Any electrons with positive energy will now have very little chance of jumping into negative-energy states, and will therefore behave like electrons are observed to behave in the laboratory. We shall have an infinite number of electrons in negative-energy states and indeed an infinite number per unit volume all over the world, but if their distribution is exactly uniform we should expect them to be completely unobservable. Only these small departures from exact uniformity, brought about by some of the negative-energy states being unoccupied, can we hope to observe.[1]

Note Dirac's final remark. Here, in January of 1930, he is entertaining what would be necessary in order to make observations of 'negative energy states' which he now felt obliged to accept, since his electron theory refused to settle for less. It is the outside possibility of such observations that Dirac is now beginning to take seriously.

In 1930 and 1931 Oppenheimer, and Heisenberg (while writing on Halogen-theory), began to speak of these small departures from exact uniformity as 'vacancies in the negative-charge continuum'. These thinkers, along with Dirac, began to think of the $+ e$ solutions of 1928 not so much as negative energy solutions, but simply as positive charge solutions. By early 1933 Blackett was confidently referring to the 'vacancies' as 'holes'.[2] These 'holes', gaps in an infinite sea of negative electricity, were taken now to be ordinary particles with positive kinetic energy and positive charge. It was already understood that they had the mass of an electron. The conclusion, therefore, was inescapable; the 'negative-energy solutions' heralded nothing other than electrons with a positive electric charge.

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[1] Dirac, *Proc. Roy. Soc.*, January 1930

[2] The idea behind the 'hole' theory is first explicitly put forward by Dirac, *Proc. Roy. Soc.*, 1930, **126**, 360; and *Proc. Camb. Phil. Soc.*, 1930, **26**, 361. In these papers the 'proton-cooking' occurred. For those to whom the conception of a positron as a 'hole in the negative-charge continuum' is difficult, the following may help. Imagine a sardine swimming through an infinite sea. This is in itself no feat of thought. But now 'reverse the Gestalt'. Instead of an infinite sea of water, imagine an infinite sea *of sardines*! They are so densely packed that they form a virtual continuum. These sardines now correspond to the most stable states for electrons ; each sardine is thus 'a negative energy particle of very high velocity'. Imagine now one sardine knocked out of its place in the sea. One would perceive a sardine-shaped vacancy in the sardine-sea, a hole which would stand out from the dense background as being something exceptional, just as a small hole in a large blackboard is immediately apparent. Suppose a sardine immediately adjacent to the hole moves into it, and that the next sardine moves into the new hole, and so also with the next, and the next. One *could* describe this as I have done. Or one *could* think of a sardine-shaped hole itself moving through the continuum. The motions of this hole would then be observed as clearly as would have been the motions of the original sardine in its infinite sea of water. And should any other sardine come skimming along the surface of the sea, it will, when it approaches

This shift of ideas percolated only slowly into the strange seas of physical thought in 1931. Thus Halpern and Thirring write:

> Dirac's system of equations refers to particles of charge $+ e$ as well as to those of charge $- e$; but this means that it has too many solutions for the charge $+ e$. As neither $+ e$ nor $- e$ has an advantage over the other, exactly half of the solutions refer to a positively charged particle and the other half to a negatively charged particle. Since in general two different solutions may be combined together, that is, a particle can pass from a state characterized by a proper function belonging to $+ e$ to the state characterized by a proper function belonging to $- e$, this signifies that according to Dirac's theory the electrons can change their sign. This is an effect for which no experimental evidence has ever been observed. It can also be shown that in the same process, namely, in the transition of the charge $+ e$ to $- e$, the sign of the total energy E must change. . . . Dirac's theory leads to the quite unintelligible result that particles with *negative mass* must exist. . . .
>
> In practice, therefore, until these difficulties are removed we shall merely retain one-half of the solutions which correspond to positive mass and subject them to comparison with experiment. That this comparison comes out satisfactorily in every respect was mentioned in the introduction. . . .[1]

Bolder is Oppenheimer's remark of 1930.

> . . . according to Dirac not all of the states of negative energy are full; there are a few gaps in the distribution for negative electrons merely at rest; and thus transitions to states of negative energy should not be quite excluded.[2]

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the hole, immediately fall into it. This will remove both that sardine, and the hole it fell into, from further observation. Only the featureless continuum will again be present; the sardine and the hole will have annihilated each other by the former's collapsing into the latter.

[1] Halpern and Thirring, *The Elements of the New Quantum Mechanics*, London, 1930-31, p. 101. And compare Condon and Mack, *Phys. Rev.*, 1930, **35**, 382: '. . . According to [Dirac's theory] electrons can exist in states of negative energy (less than $- mc^2$), as well as in usual states of positive energy (of the order of $+ mc^2$). We have no experimental evidence of electrons of this sort, and so the prediction of such negative energy electrons was regarded as a blemish in the theory. . . .'

[2] Oppenheimer, *Phys. Rev.*, 1930, **35**, 939

Even as late as 1933, W. Pauli argued that the Dirac equation was a wonderful contribution to microphysics; however, it entailed the existence of positive electrons; hence, the equation must be erroneous.[1]

Thus, the 'negative-energy' solutions, plus the rejection of their interpretation as protons, plus the 1930-32 'hole-theory', of Dirac, Heisenberg and Oppenheimer—all this constituted the gradual development of a prediction of the existence of positive electrons before they were observed. The context remained full of uncertainties and incalculables. Nonetheless, the strength of Dirac's electron theory, plus the logic of its consequences, forced theoreticians to opt for the as-yet-unobserved entity.

This development is what inclines theoreticians to characterise the discovery as a theoretical breakthrough. The reader is left to decide whether the guerdon should be given to the theoretical prediction made in the total absence of a confirming observation, or to the frank interpretation of an awkward observation in the total absence of a theory to explain it. Doubtless either undertaking required uncommon courage, skill, conviction and care. Doubtless also, both Carl Anderson and Paul Dirac possessed these characteristics to a remarkable degree. This is shown by the fact that, in entire independence of each other, and against the best-established reasons to the contrary, they took their stand on the existence of a positively charged electron. Let us conclude this section with a quotation from Dirac himself, as he writes (in 1931) of the steps leading up to his great prediction:

> The mathematical formalism at that time involved a serious difficulty through its prediction of negative kinetic energy values for an electron. It was proposed to get over this difficulty, making use of Pauli's Exclusion Principle which does not allow more than one electron in any state, by saying that in the physical world almost all the negative-energy states are already occupied, so that our ordinary electrons of positive energy cannot fall into them. The question then arises as to the physical interpretation of the negative-energy states, which on this view really exist. We should expect the uniformly filled distribution of negative-energy states to be completely unobservable to us, but an unoccupied

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[1] Pauli, *Handbuch der Physik* (1933), Chap. 2, § 5: Ubergänge zu Zuständen negativer Energie, Begrenzung der Diracschen Theorie' (pp. 242 ff). Professor Skobeltzyn remarks: 'This view [of Pauli's] prevailed in the opinion of our theoreticians up to the discovery of C. Anderson and Blackett and Occhialini.' [Letter of 22nd October 1960.] Note how this contrasts with the earlier-cited opinions of Bethe and Konopinski, to the effect that this was primarily a theoretical discovery, not an experimental one.

> one of these states, being something exceptional, should make its presence felt as a kind of hole. It was shown that one of these holes would appear to us as a particle with a positive energy and a positive charge and it was suggested that this particle should be identified with a proton. Subsequent investigations, however, have shown that this particle necessarily has the same mass as an electron[1] and also that, if it collides with an electron, the two will have a chance of annihilating one another much too great to be consistent with the known stability of matter.[2]
>
> It thus appears that we must abandon the identification of the holes with protons and must find some other interpretation for them. Following Oppenheimer,[3] we can assume that in the world as we know it, all, and not merely nearly all, of the negative-energy states for electrons are occupied. A hole, if there were one, would be a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron. We may call such a particle an anti-electron. We should not expect to find any of them in nature, on account of their rapid rate of recombination with electrons, but if they could be produced experimentally in high vacuum they would be quite stable and amenable to observation. An encounter between two hard γ-rays (of energy at least half a million volts) could lead to the creation simultaneously of an electron and anti-electron, the probability of occurrence of this process being of the same order of magnitude as that of the collision of the two γ-rays on the assumption that they are spheres of the same size as classical electrons. This probability is negligible, however, with the intensities of γ-rays at present available.
>
> The protons on the above view are quite unconnected with electrons. Presumably the protons will have their own negative-energy states, all of which normally are occupied, an unoccupied one appearing as an anti-proton.

In this passage Dirac sharply distinguishes the proton from the positron. He states that the latter, as well as the former, is in principle amenable to observation, and predicts circumstances under which positrons might be detected. He entertains seriously the ' materialisation ' hypothesis later discussed by Joliot-Curie: this is nothing less than the creation of matter out of energy, one of the boldest conjectures of all time. And he considers the possible existence of an anti-proton. All this in less than forty lines of prose!

(to be concluded)

[1] H. Weyl, *Gruppentheorie und Quantenmechanik*, 2nd edn., p. 234 (1931)

[2] I. Tamm, *Z. Physik*, 1930, **62,** 545; J. R. Oppenheimer, *Phys. Rev.*, 1930, **35,** 939; P. Dirac, *Proc. Camb. Phil., Soc.*, 1930, **26,** 361

[3] J. R. Oppenheimer, *Phys. Rev.*, 1930, **35,** 562

THE PREJUDICE OF MICRO-REDUCTION *

G. SCHLESINGER

I

THERE is a powerful tendency to take it for granted that physical order and logical order correspond to one another. We often yield to the prejudice that the physical relationship of things determines the logical relationship of propositions referring to them. It is usually assumed for instance that propositions describing the properties of a physically ' complex ' whole are logically more ' complex ' than those referring to the properties of its constituent parts. Statements about the behaviour of the ' elements ' or ' simple ' parts of a material system are regarded as if of necessity also logically more ' elementary ' or ' simple '.

In fact, however, the logical relationship between propositions is indeterminate as such. For logical relationship can only be specified in the context of some deductive system. It depends on the premises and postulates of a particular logical system whether or not, relative to it, any two given propositions are interdependent and are consistent with one another. In case both propositions are derivable in this theoretical system it entirely depends on the logical structure of this system which proposition will feature amongst the early theorems, and hence be more ' elementary ' or ' simple ' and which one amongst the more advanced or ' complex ' theorems.

To explain the nature of this deep-rooted conviction of the existence of parallel orders between the physical and the logical, and to show that it lacks objective foundation I shall first examine a recent paper by P. Oppenheim and H. Putnam—' The Unity of Science as a Working Hypothesis '.[1] I have chosen this paper with which to begin my discussion because the view it represents is a widely shared scientific ideology. It also has the advantage that, unlike most people, the authors do not merely take the correspondence for granted but state it in clear terms and boldly support it with arguments. This renders their thesis more vulnerable to criticism, and it becomes a

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* Received 27. ix. 60

[1] *Minnesota Studies in the Philosophy of Science*, vol. 2, pp. 3-35

comparatively easy task to expose the sentiments voiced by them, as objectively unfounded.

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Oppenheim's and Putnam's major thesis is a familiar one: the sciences can be arranged in a hierarchy ordered according to the complexity of the entities with which they deal. The authors divide these entities into six groups corresponding to six levels of complexity. Entities belonging to a given level are comprised of entities belonging to the level immediately below them: Level 6—Social Groups; Level 5—(Multicellular) Living Things; Level 4—Cells; Level 3—Molecules; Level 2—Atoms; Level 1—Elementary Particles. At present, entities of each level are described by predicates of which some belong exclusively to that level. The entities are governed by laws whose study constitutes, in part, the various special disciplines. It is hoped, however, that the special concepts and laws associated with the different levels will eventually be disposed of, and the various sciences dealing with them be replaced by a single unifying science.

The eventual unification is envisaged by the authors as taking place by a method they have named ' micro-reduction '. It consists in the breaking up of a system into its elements and explaining the properties of the whole in terms of the properties of its parts. The properties of entities of level $n + 1$ will therefore be explained in terms of the properties of entities of level n, and these in turn will be explained in terms of the properties of objects belonging to level $n - 1$. Thus the only science which will remain, after the reduction of all the others to its level, will be the science of sub-atomic physics.

The authors' argument in favour of the adoption of their programme may be divided into three parts, (*a*) that the unity of science is a desirable working hypothesis, (*b*) that the possible objections to its attainability can be met, and (*c*) that there is positive evidence to support the claim that it will be attained.

The essence of (*a*) is that the unification of science will bring about an enormous simplification, and no one will want to quarrel with this. That the correlation of the laws governing various phenomena renders science more manageable and simple is a truism. This creates no special grounds, however, on which to prefer micro-reduction to any other method of unification. The essential prerequisites for the correlation of the laws governing the behaviour of different kinds of entities are: (1) that the relevant properties of the parts be governed by regularities expressible by a group of propositions which I shall denote by P_1, (2) that the relevant properties of the whole be governed by regularities expressible by a group of propositions which I denote by P_2, and (3) that we be able to construct a theory consisting of a reasonable number of premises and postulates (a small part of P_1 and P_2 may feature amongst them) in which P_1 and P_2 can logically be derived. Whether this theory is of the type T_1, namely that P_1 features amongst the earlier theorems of the deductive system, or of the type T_2, in which case P_2 constitutes the elementary part of the theory, has no bearing on the amount of economy achieved. The simplification introduced by T_2 (by macro-reduction) may be just as great as by T_1 (by micro-reduction).

Of the arguments of interest that come under (*b*), one deals with the objection that their programme neglects the fact of emergence. According to the thesis of emergence when objects of a given level combine to form wholes belonging to a higher level, the new properties that emerge are not reducible to the properties the elements possess when separated. Oppenheim and Putnam argue, against this, that though in practice there are many properties that seem emergent, no one has so far produced positive arguments to show that reduction is impossible in principle. Our inability at the moment to ' micro-reduce ' all phenomena should not be construed as positive evidence against the working hypothesis that this will eventually be possible.

Their stand against the dogma of emergence seems very reasonable. It should be pointed out, however, that Oppenheim and Putnam, like almost everyone else who has discussed the concept of emergence, take it for granted that properties of wholes are either emergent or, if not, they are what we now call ' micro-reducible '. But, in fact, the proper alternatives are: the thesis of emergent phenomena (or the incorrelativity of phenomena) on the one hand, and the thesis of the connectibility of phenomena on the other. For correlation may be achieved in a variety of ways. In other words, if no theory can be constructed in which both P_1 and P_2 are derivable, then the properties of the whole are to be regarded as emergent. On the other hand, if such a theory can be constructed, then no matter whether it is of type T_1 or T_2, the properties of the whole are not emergent but connectible to those of its elements.

Of the arguments that come under (*c*)—the positive arguments in favour of the authors' plan—the one based on the theory of universal evolution is of major significance and interest. Oppenheim and Putnam quote the views of a large number of authorities to show that their table of reductive levels represents a hierarchy of evolutionary levels. Whatever belongs to a higher level in their scheme came into existence at a later stage in history. They devote a section of their paper to showing that human beings lived as isolated individuals before societies came into existence; that, at an early time, only inorganic matter existed; and that there was even a period, lasting from five to thirty minutes after our universe began, in which there was nothing but subatomic particles. This, they contend, provides support for their thesis, for:

> Let us, as is customary in science, assume causal determination as a guiding principle; i.e. let us assume that things that appear later in time can be accounted for in terms of things and processes at earlier times. Then, if we find that there was a time when a certain whole did not exist, and that things on a lower level came together to form that whole, it is very natural to suppose that the characteristics of the whole can be causally explained by reference to these earlier events and parts; and that the theory of these characteristics can be microreduced by a theory involving only characteristics of the parts.[1]

This is a striking example of the tendency to identify causal order with logical order. Let me indicate, by means of a simple concrete example, where the error lies.

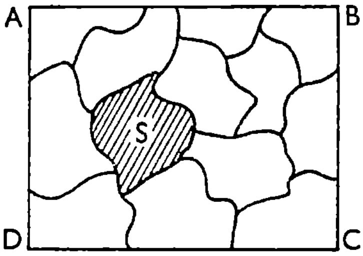

Fig. 1.

The uniform reflecting surface ABCD consists of curvilinear surfaces of which S is of typical shape. We may logically derive the value of the total reflection for ABCD from our knowledge of the separate values for all the S-surfaces, by assuming or postulating that the property of total radiation is additive.

There is, however, an alternative, indirect method whereby we may arrive at the value of the total reflection of ABCD but not in terms of any of the properties of its constituent elements, the S-surfaces.

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[1] Op. cit., p. 15.

If we know the reflective power of the kind of surface possessed by ABCD, we may use the formula: total reflection = reflective power × area. To calculate the area we employ the simple formula provided in Euclidean geometry.

If the S-surfaces are of such shape that the value of their area is derivable in Euclidean geometry, then the properties of total radiation of the whole and its parts may also be unified by the use of our last method. This unification is obtained within the framework of a typical T_2 theory. The theorem giving the area of an S-figure would be an advanced one presupposing many earlier theorems, including all those required for the demonstration of the formula: area of rectangle = length × breadth. In the context of Euclidean geometry, that aspect of the complex whole which is known as area is more elementary than that of its physical elements, the S-figures. The propositions about the respective values of the total reflection are not bridged by micro-reduction.

Now, no argument from evolution could show that in the T_2 theory the principle of causality had been violated. Imagine that at an early stage of evolution the whole universe consisted of S-surfaces which at a later stage combined to form the rectangular surfaces that are now in existence. It would clearly be pointless to argue that since historically the curvilinear figures preceded the rectangular ones, and since the latter were caused to come into existence by the conglomeration of the former, no T_2 theory can truly account for their present properties. The logical order of statements about the properties of different entities is independent of their physical order of succession. The former is determined solely by the structure of the unifying theory.

The argument from universal evolution has, however, a curious suggestive power. I have heard it defended in the following manner. Suppose for example that the properties of inanimate matter were explained by a T_2 theory, that is, they were macro-reduced and explained in terms of the properties of organic cells. This would imply that the behaviour of living cells determined the behaviour of molecules. Assuming that a cause cannot act except when it is and where it is, you would be forced to conclude that before there was life on earth—so that cells could not determine the behaviour of molecules—the behaviour of inorganic matter was indeterminate!

But this is merely a restatement of the same confusion between causal and logical determination. There is no reason why propositions about the behaviour of non-existent entities (and if you like of entities that will never exist) should not, in the context of a certain theory, logically determine or imply propositions about the behaviour of existing entities. Our firm belief in determinism would in no way diminish if we claimed that the behaviour of molecules was always governed by strict laws; however, these laws and the laws governing the behaviour of cells cannot be unified unless by a theory in which the latter constitute the more elementary theorems.

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A further argument of Putnam and Oppenheim is as follows. Suppose that B_1 is that branch of science which deals with entities, E_1 and B_2 that branch which deals with entities E_2, then:

> . . . it seems very doubtful to say the least, that a branch B_2 could be reduced to branch B_1, if the things in the universe of discourse of B_2 are not themselves in the universe of discourse of B_1, and also do not possess a decomposition into parts in the universe of discourse of B_1 (they don't speak about the same thing).[1]

Yet, consider the way the Boyle-Charles Law was ' micro-reduced ' to the laws of mechanics within the kinetic theory of gases. How could this be done, when the Boyle-Charles Law ' speaks about ' concepts like ' temperature ' that are not to be found in Newtonian Mechanics? As Ernest Nagel so clearly explained,[2] such reductions are accomplished by the introduction of a postulate of correlation, in this case that the thermodynamical ' temperature ' is proportional to the mechanical ' mean kinetic energy '. It is a mistake to think that, just as physical analysis will reveal a gas to consist of molecules, logical analysis will reveal that ' temperature ' means ' mean kinetic energy '. The laws governing macroscopic entities are not by themselves entailed by the laws governing microscopic entities even though the former entities physically result from the latter. This, of course, is true even in cases where, unlike the case of mechanics and thermodynamics, P_1 and P_2 employ the same set of concepts. Even the innocuous statement that the weight of a gas is the sum total of the weight of its constituent parts requires the postulate that weights are additive.

Certain features of common language may also be responsible for our thinking that, just as, say, societies possess a decomposition into individuals so do the concepts of sociology possess a natural decomposition into the concepts of individual psychology. I have already alluded to the suggestive nature of such double-meaning terms as 'elementary', 'simple' and 'complex'. To take a more conspicuous example, consider the term 'analysis'. According to the Oxford English Dictionary, 'analysis' is the act of proving a proposition by 'resolving' it into simpler propositions already proved or admitted. In the physical sense, the analysis of an aggregate is its resolution into its component particles. Both processes are denoted by the same word. Yet we must appreciate that this does not constitute a guarantee that they are parallel processes.

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[1] Op. cit., p. 8

[2] ' The Meaning of Reduction in the Natural Sciences ', *Science and Civilization*, edited by R. C. Stauffer, Wisconsin, 1949, pp. 99-135

'Speaking about the same thing' then is not ensured by the presence of particular physical set-up to which propositions refer. It is a logical feature varying with the theories (and their sets of postulates) in the context of which these propositions are viewed. There is no warrant therefore to conclude

> Thus one cannot plausibly suppose—for the present at least—that the behaviour of inorganic matter is explainable by reference to psychological laws; for inorganic material does not consist of living parts.[1]

for inorganic matter may not physically consist of living parts yet the laws of physics may be analysed into the laws of biology within the context of a theory in which the latter form the elementary parts.

4

It would seem perhaps that one might yet take a more modest stand in defending the thesis of micro-reduction. One might be prepared to admit that there are no *a priori* reasons to say that the properties of the whole and its parts, if at all connectible, are more likely to be connectible by a T_1-theory than by a T_2-theory and still wish to argue that it is more desirable that they be so connected. Thus even though there is no guarantee that in any particular case a T_1-theory can be constructed we should never abandon the search for such theory because of its superiority over a T_2-theory.

In support of this last claim one would cite what are commonly agreed the essential features of a good theory. A good deductive system, it is said, should have a minimum number of independent axioms or premises which should entail a maximum number of theorems. In a T_1-theory statements about the behaviour of the elements form the premises, which are but a few, and the more numerous statements describing the diverse behaviour of the aggregate are the entailed theorems. A T_1-theory therefore better confirms to the requirements of a good theory.

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[1] *Minnesota Studies in the Philosophy of Science*, vol. 2, p. 8

But the unwarranted assumption that has been introduced into this argument is the quite common notion that physical elements can never have more properties than the complex system that their assembly gives rise to. For an aggregate, it is argued, must at least possess all the properties of its parts and in normal cases some additional ones as well. A macroscopic body, for example, has mass, momentum, kinetic energy, but in addition, it has properties like temperature and viscosity which its microscopic elements do not possess.

This idea, which is perhaps most dramatically illustrated by the perennial search for the ultimate simple elements in nature, has, however, no logical basis. One can even demonstrate empirically that it is possible for elements to possess properties not shared by their aggregates. For example, portions of a system each by itself may sometimes have the property of being electrically charged hence possessing a disposition to exert a force upon uncharged bodies. When, however, they are put together, to form a system, their assemblage may become electrically neutral and devoid of such disposition.

Another example: curvilinear slabs have many dispositions rectangular slabs lack. One such disposition is to start rolling with different rates of acceleration when stood on different points on their circumference. But, in some cases, slabs which form the parts of a larger system may possess the property of having curvilinear edges, a property lacked by the whole formed by their conglomeration (as illustrated by Figure 1).

Apart from this we must realise also that the question of the relative merits of a T_1-theory and a T_2-theory is independent of the question whether the parts or the compound possess the larger number of significant properties. Not all the properties of the whole need to form the independent unproven and initial theorems of our deductive system, only some of them. The rest may be derivable from those and so may the properties of the part. Thus even if it were true that the properties of a complex system were always more numerous than those of its constituent elements, the argument introduced in this section to defend the prejudice of micro-reduction would be ineffective.

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The conclusion reached is that there are no objective grounds on which to maintain that a T_1-theory will naturally be more readily available, or is more strictly in conformity with a deterministic view, or is for reasons of economy or elegance in any way more desirable, than a T_2-theory. There is just no reason why we should seek micro-reduction in preference to macro-reduction. Nevertheless it is a highly plausible claim that if the unification of science is eventually to be achieved, it will be achieved by micro-reduction. For the universal and deep-seated conviction in the thesis of micro-reduction will probably ensure that this will happen.

This conviction is reflected in common language, but it has also explicitly been voiced by philosophers of all ages. From Aristotle who said:

> . . . the compound should always be resolved into the single elements or the least part of the whole.[1]

till Wittgenstein who said:

> Every statement about complexes can be analysed into a statement about their constituent parts, and into those propositions which completely describe the complexes.[2]

philosophers assumed without much ado that the properties of the whole could and should always be explained in terms of those of their constituent elements, or what it all boils down to, that there is a natural intrinsic correspondence between physical order and logical order.

This conviction has had a tremendous influence on the course of science. Although macro-reduction has from time to time been attempted with some notable results[3] the vast majority of those theories in science which connect the behaviour of wholes with those of their parts are micro-reductive. The long history of the atom illustrates in a special way the great power of this conviction. In this case, the existence of a micro-reductive theory based on the few and simple properties of the parts in terms of which the manifold properties of the whole will be explained, was assumed, long before the existence of those parts could with any degree of credibility be maintained.

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[1] *The Basic Works of Aristotle*, New York, 1941, p. 1127

[2] L. Wittgenstein, *Tractatus Logico-Philosophicus*, London, 1922, 2.0201

[3] E.g. Stevin's derivation of the law of the composition and resolution of forces acting on a single particle from the behaviour of an endless chain, i.e. an aggregate of such particles. For the discussion of this and some other examples, see my 'Two Approaches to Mathematical and Physical Systems', *Philosophy of Science*, **26**, No. 3.

The firm universal belief in the efficacy of micro-reduction, then, if nothing else, may bring about the realisation of the prognostications of Oppenheim and Putnam. For, as in the past, so in the future, we shall persist in exploring all the avenues that lead to the micro-reduction of phenomena.

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ARGUMENTS ABOUT MEANINGLESSNESS *

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SOME philosophers have tried to set up a principle that would enable them to dismiss certain kinds of utterance as literally meaningless. In this paper we will show that some of the arguments used in this connection were unsound. From a logical point of view, the weaknesses in these arguments are of some interest. Philosophically the arguments may be out of date, but even if this is so their defects should still be noted, because philosophers have been known to retrace their steps.

'A sentence has literal meaning if and only if the proposition it expresses is either analytic or empirically verifiable.' Ayer says that it might be argued that the Principle of Verification, formulated in this way, is otiose, because a sentence that expresses any sort of proposition is meaningful. In order to deal with this difficulty, he decides to use the word 'statement' in a new way.[1] His proposals are as follows: any form of words that is grammatically significant is a sentence; every indicative sentence expresses a statement, and some but not all statements are propositions. Indicative sentences that express statements that are propositions are literally meaningful, and those that express statements that are not propositions are literally meaningless. Ayer retains the rule that all propositions and only propositions are either true or false, and for brevity he describes statements that are not propositions as being literally meaningless. Thus he is committed to saying that every statement has one of the three values: true, false, and literally meaningless.

Carl Hempel, in discussing the Principle of Verification, does not speak of statements or propositions but only of sentences, and for him it is indicative sentences that are true, false, or cognitively meaningless.[2]

In this paper we will usually abbreviate 'literally meaningless' and 'cognitively meaningless' to 'meaningless', and since the alleged difference between indicative sentences and statements is irrelevant, we

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* Received 4.x.60.

[1] A. J. Ayer, *Language, Truth and Logic*, 2nd edition, London, 1946, p. 8

[2] Carl G. Hempel, 'Problems and Changes in the Empiricist Criterion of Meaning,' *Revue internationale de Philosophie*, II, 1950, 41-63

will refer to whatever are true, false, or meaningless, as statements. The capital letters ' O ', ' P ', ' Q ', ' S ', sometimes with suffixes, will be used as the names of statements, and P $\vee$ Q, for example, is the name of the statement which is the disjunction of P and Q.

In talking about statements that are true, false, or meaningless, we make meta-language statements that are either true or false. The implication statements with which we shall be concerned, are such that their truth depends only upon the validity of truth functional schemata. For example, the statement ' Given *any* two statements P and Q, Q follows from P . P $\supset$ Q ' is true if and only if the schema $p \,.\, p \supset q \,. \supset q$ is valid. We assume that the validity of the appropriate truth functional schema is a condition not only necessary but also sufficient for one statement following from, being deducible from, being entailed by, or being a logical consequence of another. Some people hold that the validity of a material conditional is not a condition sufficient for an entailment, but here we use ' entails ' in the way in which it is used in the arguments that we examine, and in this context our assumption is justified.

2 First we will show that some arguments against certain formulations of the Principle of Verification are unsound, because in them a consequence of using ' statement ' in a new way is overlooked.

2.1 One formulation of the principle was that ' a statement is verifiable, and consequently meaningful, if some observation statement can be deduced from it in conjunction with certain other premises, without being deducible from those other premises alone '[1] Ayer accepted the following argument to the effect that this allows meaning to any statement whatever. For any statement S, the observation statement O follows from S and S $\supset$ O without following from S $\supset$ O alone, and consequently by the above criterion, S is meaningful. But a necessary condition for O following from S and S $\supset$ O is that

$$p \cdot p \supset q \cdot \supset q \qquad \text{(i)}$$

comes out true for every interpretation of p and q. This schema is valid in the classical propositional calculus, but in this calculus letters may be interpreted only as being either true or false. If S is a statement in Ayer's sense of ' statement ', then it is not a possible interpretation of a letter in a schema in a two-valued logic, and to treat it as such is to beg the question by supposing that it is either true or false, and not meaningless.

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[1] A. J. Ayer, op. cit., p. 11

2.2 One of the criteria discussed by Hempel is that a statement is meaningful if and only if it is either analytic or self-contradictory, or follows logically from some finite and logically consistent set of observation statements (O).[1] We are asked to suppose that S and Q are statements that are not analytic or self-contradictory, and that S follows logically from O, and is consequently meaningful, but that Q is not meaningful. Hempel then argues that $S \vee Q$ is meaningful despite its meaningless component, because being a consequence of S, it is also a consequence of O, his objection being that an adequate criterion should rule out such statements as $S \vee Q$. Here it is being assumed that

$$p \supset q . \supset : q \supset r . \supset . p \supset r \qquad \text{(ii)}$$

$$p \supset . p \vee q \qquad \text{(iii)}$$

are valid in a logic in which the letters may be interpreted as being true, false or meaningless.

2.3 Hempel's argument is a part of what J. W. N. Watkins calls the tacking paradox.[2] Suppose we accept (1) that if a compound statement has a meaningless component, then it is meaningless, and (2) that if a statement is a logical consequence of a meaningful statement then it is meaningful. This, according to Watkins, leads to a contradiction, for $S \vee Q$ is meaningless because its component Q is meaningless, and it is meaningful because it is a logical consequence of the meaningful statement S. This is not a paradox. $S \vee Q$ would be a logical consequence of S and thus meaningful (by 2), only if $p \supset . p \vee q$ were true for all interpretations of p and q. But (by 1) $p \supset . p \vee q$ is not true if p or q is interpreted as meaningless. Thus it is possible to accept without contradiction or paradox that if a compound statement has a meaningless component, then it is meaningless (1), and that if a statement is not meaningful, then it is not a logical consequence of a meaningful statement (contrapositive of 2).

2.4 Ayer replaced the criterion mentioned in 2.1 by the following recursive one.[3] S is directly verifiable if and only if it is an observation statement, or such that, O_1 S entails O_2, where O_1 and O_2 are observation statements such that O_1 does not entail O_2. S is indirectly verifiable if and only if S P entails a directly verifiable statement not entailed by P alone, where P is a conjunction of statements each one of which is analytic, directly verifiable or independently indirectly verifiable. A literally meaningful statement is either analytic or self-contradictory, or directly or indirectly verifiable.

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[1] Carl G. Hempel, op. cit., p. 45

[2] J. W. N. Watkins, 'When are Statements Empirical?', *The British Journal for the Philosophy of Science*, 40, 1960, 304

[3] A. J. Ayer, op. cit., p. 13

Church[1] claimed to show of any statement S that either it or its negation is directly or indirectly verifiable (under this definition), provided that there are three observation statements O_1, O_2, O_3, no one of which entails any other. $\bar{O}_1 O_2 \vee O_3 \bar{S}$ is directly verifiable because with O_1 it entails O_3. Since S and $\bar{O}_1 O_2 \vee O_3 \bar{S}$ together entail O_2, either S is indirectly verifiable, or $\bar{O}_1 O_2 \vee O_3 \bar{S}$ alone entails O_2, in which case $O_3 \bar{S}$ entails O_2, and $\bar{S}$ is directly verifiable.

But for the first two entailments to hold generally it is necessary that

$$p . \bar{p} q \vee r \bar{s} . \supset r \tag{iv}$$

is a valid schema. And the last step in the argument depends upon the validity of

$$\bar{p} q \vee r s . \supset q : \supset . r \bar{s} \supset q \tag{v}$$

These schemata are valid in the classical propositional calculus, but in this calculus the letters may be interpreted only as being either true or false. Thus what Church has shown is that any true-or-false statement, that is to say, any meaningful statement, is such that either it or its negation is verifiable, which does not conflict with Ayer's recursive criterion.

3 Although the arguments discussed in 2 are unsound, it does not follow that the formulations of the Principle of Verification against which they are directed are satisfactory, particularly as nothing has been said to indicate that the formulations are not confused in just the way that the arguments are. In order to reassess both, it is necessary to take account of the new way in which ' statement ' is being used, by setting up a statement calculus in which the letters may be interpreted as true (1), false(0) or literally meaningless ($\frac{1}{2}$), and going on to see if the relevant schemata are valid in this calculus. This is not trivial, for if the notion of negation is extended so that the negation of a meaningless statement is meaningless, and if the rule is retained that a schema is valid if and only if it comes out true for all interpretations of its letters, then no matter how the relevant connectives are defined, some schemata that are valid in the classical propositional calculus will be invalid in the three-valued calculus (see below).

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[1] Alonzo Church, Review of Ayer's *Language, Truth and Logic* 2nd edn., in *The Journal of Symbolic Logic*, I, 1949, 53

In setting up this calculus we define connectives by means of matrices. Since we are merely extending existing notions of disjunction, conjunction and the material conditional in order to deal with statements that may be meaningless, we retain the values assigned to functions when their components are either true or false, and have only to assign values to functions when one or more of their components are meaningless. If F is a connective already defined for two values, then the values of a, c, g and i in

$$\begin{array}{c|ccc} \mathrm{F} & 0 & \frac{1}{2} & 1 \\ \hline 0 & a & b & c \\ \frac{1}{2} & d & e & f \\ 1 & g & h & i \end{array}$$

are retained. If ' F ' is the material conditional sign, for example, then a, c, g and i are 1 1 0 1 respectively. We define such connectives for three values by specifying b, d, e, f and h. We denote the resulting matrix by the appropriate letter in the Polish notation followed by the values of b, d, e, f and h. Thus C 1 1 1 1 0 denotes the matrix

$$\begin{array}{c|ccc} \mathrm{C} & 0 & \frac{1}{2} & 1 \\ \hline 0 & 1 & 1 & 1 \\ \frac{1}{2} & 1 & 1 & 1 \\ 1 & 0 & 0 & 1 \end{array}$$

which defines the material conditional for three values.

What values are to be given to b, d, e, f and h is not obvious. Rosser and Turquette [1] have suggested that if truth values are regarded as indicating degrees of assertability, then a conjunction may be generalised as the function that is just as assertable as the least assertable of its components, and a disjunction as the function that is just as assertable as the most assertable of its components. But this method cannot be applied here, because whether a meaningless statement is to be regarded as being more or less assertable than a false one, is arbitrary.

A very simple way of producing a three-valued calculus would be to adopt the suggestion 2.3 (1) that a function is meaningless if it has one or more meaningless components (i.e. $b=d=e=f=h=\frac{1}{2}$ for all connectives), and to accept as valid any schema which does not come out false for any interpretation of its letters. All schemata valid in the classical propositional calculus would be valid in this system, but its adoption would lead to the following difficulty. Although the schema (iii), for example, is valid in this system, from the truth of S, it follows that $S \vee Q$ is not false, but not that it is true. Also the substitution of statements for letters in a valid schema does not always give a true statement. Because of this second difficulty, we will retain the rule that a schema is valid if and only if it comes out true for all interpretations of its letters (R).

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[1] J. B. Rosser and A. R. Turquette, *Many valued Logics*, Amsterdam, 1952, p. 15

A method more suitable in this context is that used by Dienes.[1] He has worked out which of certain important characteristics of negation, disjunction, conjunction, and the material conditional and biconditional are retained, and which lost, when the matrix definitions of these connectives are extended in every possible way to cover a third value. Here his results are accepted and some of their consequences worked out and applied.

Unlike Dienes, who is not particularly concerned with interpretations of the third value ($\frac{1}{2}$), we intend to interpret it as 'literally meaningless', and this enables us to choose one of the three possible extensions of negation. Although from some criteria of significance it follows that the negations of some meaningful statements are meaningless, this consequence has, as far as we know, always been regarded as a defect. This being so, we will take it that only the negation of a meaningless statement is meaningless, and adopt as negation the connective that Dienes calls N. He has shown that there are only two ways of extending conjunction and disjunction so that they continue to be commutative, associative, mutually distributive and de Morgan transforms of each other, these being $A\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}$, $K\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}$ and $A\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}\,1\,1$, $K\,0\,0\,\frac{1}{2}\,\frac{1}{2}\,\frac{1}{2}$. We now have to extend the material conditional in such a way that when combined with one of these pairs, it retains its more important characteristics. In adopting N, and in keeping the rule (R), we have precluded the possibility of retaining as valid all the schemata that are valid in the two-valued calculus, and consequently we have to decide what we require of an extension of the material conditional.[2] Dienes describes as 'the usual requirements' the validity of six schemata including (i), (ii), and (iii). If we add (iv) and (v) to his list, we have:

$$\begin{array}{ll} \text{(i)} & p \,.\, p \supset q \,.\supset\, q \\ \text{(i}'\text{)} & p \,\supset : p \supset q \,.\supset q \end{array} \quad \left\{\begin{array}{l} \textit{modus} \\ \textit{ponens} \end{array}\right.$$

$$\begin{array}{ll} \text{(ii)} & p \supset q \,.\supset: q \supset r \,.\supset.\, p \supset r \\ \text{(ii}'\text{)} & p \supset q \,.\, q \supset r \,.\supset.\, p \supset r \end{array} \quad \left\{\begin{array}{l} \text{hypothetical} \\ \text{syllogism} \end{array}\right.$$

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[1] Paul Dienes, 'On Ternary Logic', *The Journal of Symbolic Logic*, **2**, 1949, 85–94

[2] Paul Dienes, op. cit., 92, n. 7

(iii) $.\, p \supset .\, p \vee q$

(iv) $p \,.\, \bar{p}q \vee r\bar{s} \,.\supset r$

(v) $\bar{p}q \vee r\bar{s} \,.\supset q : \supset .\, r\bar{s} \supset q$

(vi) $p \supset q \,.\supset .\, \bar{q} \supset \bar{p}$ contraposition

The following points also have a bearing upon definitions of the material conditional. Let X $\supset$ Y be a valid schema, and let P and Q be statements obtained from X and Y respectively by the same interpretation of letters. If in the definition of the material conditional $h = 1$, then from the premise that P is true we cannot conclude that Q is true, but only that it is not false. If $b = 1$ or $h = 1$, then from the premise that P is meaningful we cannot conclude that Q is meaningful. Thus in order to retain the rule that any statement implied by a true statement is true, we must, in defining the material conditional, give to h a value other than 1. The value of b is not as important, for although if $b = 1$ and $h \neq 1$, Q may be a meaningless statement when P is false, since the false antecedent cannot be asserted, the meaningless consequent cannot be detached.

The sets of definitions

$$(\alpha)\ \mathrm{N},\ \mathrm{A}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2}\,1\,1,\quad \mathrm{K}\,0\,0\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2},\quad \mathrm{C}\,1\,1\,1\,1\,0$$
$$(\beta)\ \mathrm{N},\ \mathrm{A}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2}\,1\,1,\quad \mathrm{K}\,0\,0\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2},\quad \mathrm{C}\,1\,1\,1\,1\,\tfrac{1}{2}$$

satisfy all of the above requirements except the validity of (vi). The requirement that $b \neq 1$ in C $b\,d\,e\,f\,h$, is not satisfied either. The set

$$(\gamma)\ \mathrm{N},\ \mathrm{A}\,0\,0\,0\,1\,1,\quad \mathrm{K}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2}\,\tfrac{1}{2},\quad \mathrm{C}\,\tfrac{1}{2}\,1\,1\,1\,\tfrac{1}{2}$$

in which $b \neq 1$ in C $b\,d\,e\,f\,h$, satisfies all of the above requirements except the validity of (iv) and (vi). With this set, however, conjunction is not distributive with respect to disjunction, and conjunction and disjunction are not de Morgan transforms of each other.

We have shown that it is possible to define connectives for three values in such a way that the following statements are true when S and Q are statements which may be true, false, or meaningless, and O, O_1, O_2, O_3 are observation statements that are either true or false:

(vii) For any statements S, the observation statement O follows from S and S $\supset$ O without following from S alone.

(viii) For any statements S and Q, if S follows logically from O, then S $\vee$ Q, being a consequence of S, is also a consequence of O.

(ix) For any statement S, $\overline{O_1}\, O_2 \vee O_3\, \overline{S}$, with O_1, entails O_3.

4 We will now reassess the arguments in which these statements were made.

4.1 If (i) is valid, then it comes out true for all interpretations of its letters (e.g. $p = \frac{1}{2}$, $q = 1$). So although O follows from S and S $\supset$ O, from the fact that O, being an observation statement, is either true or false, it does not follow that S is not meaningless. Thus the argument in 2.1 shows, not that the principle, formulated in one way, allows meaning to any statement whatever, but that this formulation of the principle is not an adequate criterion of significance, since it is satisfied by statements that are meaningless.

If (i) is invalid, then (vii) is false, and the argument in which it is a premise is not conclusive. Thus whether (i) is valid or not, the argument 2.1 does not prove what it was taken to prove.

4.2 According to the criterion mentioned in 2.2, if a statement P is a logical consequence of S, then if S is a meaningful statement, P is also meaningful. This is the case only when $b \neq 1$ and $h \neq 1$ in C $b\,d\,e\,f\,h$, (γ) being an example of a set of definitions of this type which secures the validity of (ii) and (iii). If such definitions are adopted, then although it is true that if S is a meaningful statement, then S $\vee$ Q is meaningful, even when Q is meaningless, this is harmless. It is harmless because the definitions adopted are such that Q, being meaningless, is a logical consequence only of other meaningless statements, none of which can be a premise in an argument.

If (α) or (β) ($b = 1$, $h \neq 1$ in C $b\,d\,e\,f\,h$) are adopted, then S $\vee$ Q is a true statement if S is true and Q meaningless, and S $\vee$ Q is a meaningless statement if S is false and Q meaningless. In the first case, Q, being meaningless, cannot be deduced from true premises. In the second, although S Q is a logical consequence of S, since a false antecedent cannot be asserted, the meaningless consequent cannot be detached.

It appears that there is no reason why S $\vee$ Q should be ruled out as meaningless when S is meaningful and Q is not, but there are, as Hempel points out, other objections to the criterion that he is considering.

4.3 We have already shown that it is possible to accept both (1) and (2) in 2.3. We are now in a position to show that they are independent of each other.

To adopt the set of definitions (γ) is to reject (1) and accept (2).

To adopt the definitions (α) or (β) is to reject both (1) and (2).

Suppose that we adopt (1) and, despite the complications noted in section 3, give up (R), accepting as valid any schema which does not come out false for any interpretation of its letters. The schema $p \supset . p \vee q$ is valid, and $p \vee q = \frac{1}{2}$ when $p = 1$ and $q = \frac{1}{2}$. So P $\vee$ Q is a logical consequence of the statement P, even when P is a meaningful statement and $P \vee Q$ a meaningless one. Thus although (1) is adopted, (2) has been rejected.

4.4 If we adopt definitions (α) or (β), then (iv) and (v) are valid, and Church's argument does show of any statement that either it or its negation is verifiable. But since (iv) is valid, it comes out true for all interpretations of its letters, and although if $\overline{O}_1\ O_2 \vee O_3\ \overline{S}$ does not entail O_2, then S is indirectly verifiable, S may still be meaningless. If, on the other hand, $\overline{O}_1\ O_2 \vee O_3\ \overline{S}$ and thus $O_3\ \overline{S}$ does entail O_2, then $\overline{S}$ is directly verifiable. But in this case too $\overline{S}$ may be meaningless for if $\overline{S}$ is $\overline{O}_3\ O_1 \vee O_2\ \overline{Q}$, then $O_3\ \overline{S}$ entails O_2, and if O_1, O_2, O_3 are true and Q meaningless, then $\overline{S}$ is meaningless.

If we define connective in such a way that if $O_3\ \overline{S}$ entails O_2 then $\overline{S}$ cannot be meaningless, then (iv) is not valid. This is proved as follows:

Let $X\,Y \supset Z$ be a valid schema

It is required that

$$\text{if } X \neq \tfrac{1}{2} \text{ and } Z \neq \tfrac{1}{2} \text{ then } Y \neq \tfrac{1}{2}$$
$$\text{i.e. if } X \neq \tfrac{1}{2} \text{ and } Y = \tfrac{1}{2} \text{ then } Z = \tfrac{1}{2}$$

The only definitions that satisfy this requirement, and are extensions of definitions in the two-valued calculus are those in which

$$b = h = \tfrac{1}{2} \quad \text{in K}\, b\, d\, e\, f\, h \tag{1}$$
$$d \neq 1, f \neq 1 \text{ in C}\, b\, d\, e\, f\, h \tag{2}$$

Let negation be N

In (iv) it is required that

$$\text{if } p \neq \tfrac{1}{2} \text{ and } r \neq \tfrac{1}{2} \text{ then } \bar{p}\, q \vee r\, \bar{s} \neq \tfrac{1}{2}$$

Because of (1) and (2) this can be secured for all values of q and s only if

$$b \neq \tfrac{1}{2},\ d \neq \tfrac{1}{2},\ e \neq \tfrac{1}{2},\ f \neq \tfrac{1}{2},\ h \neq \tfrac{1}{2}, \text{ in A}\, b\, d\, e\, f\, h \tag{3}$$

If in (iv) $p = \frac{1}{2}$ and $r \neq \frac{1}{2}$

Since from (3) $\bar{p}\, q \vee r\, \bar{s} \neq \frac{1}{2}$

then by (1) $p \,.\, \bar{p}\, q \vee r\, \bar{s} = \frac{1}{2}$

But if $p \,.\, \bar{p}\, q \vee r\, \bar{s} = \frac{1}{2}$ and $r \neq \frac{1}{2}$

then by (2) $p \,.\, \bar{p}\, q \vee r\, \bar{s} \,.\! \supset r \neq 1$

i.e. (iv) is not valid.

If (iv) is not valid then (ix), which is one of Church's premises, is not true, and if (v) is invalid, then his argument is invalid. In either case the argument against Ayer's recursive criterion fails. If, on the other hand, we adopt definitions such that (iv) and (v) are valid, then it follows by Church's argument, that any statement is such that either it or its negation is verifiable, and by the arguments given above that a verifiable statement may be meaningless. In this case the recursive criterion stated in 2.4 is shown to be inadequate.

5 An attempt to formulate a general distinction between meaningful statements and meaningless ones is pointless unless some statements are meaningful and others not. If we accept that statements may be meaningless as well as true or false, then we must extend our notions of implication and the other logical relations to take account of this. Whether a particular argument is effective against a given criterion of significance or not, depends upon the way in which we extend these notions. The number of ways in which we might do this is very large, and an examination of arguments will not help us to choose between them, because our arguments normally proceed upon the presupposition that our assertions are either true or false. We can choose our definitions of disjunction, conjunction and the material conditional as applied to statements that are true, false or meaningless, only after we have decided which arguments are to remain valid and which not. If for example, we wished to retain the criterion in 2.4, we would define connectives in a way that would make Church's argument invalid. This would prevent confusions of the kind discussed in section 2, but it would do nothing to settle disagreements about meaningfulness, for if someone accepted as meaningful a statement meaningless by our criterion, he would reject the criterion, and would then have no reason to accept the definitions.

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REVIEWS

THE SCIENCES AND DEITY

1 *Introduction*

MAN (under God) had for long been conceived as lord of the universe. But with the advance of modern science in all its branches, man has been dethroned. He has shrunk in significance. Copernicus, Darwin, Marx, and Freud, in particular, have more than ' taken him down a peg or two '. It is good of course in that many appallingly naïve superstitions have been swept away and a race of ' sensible ' humanists comes into being but they are depressed by the materialism that seems to be the natural result of the apparent unimportance of man. They must have a god somewhere, somehow, so that they can have Something to *look up to.* Luckily there seems to be a solution. Plain ' evolution ' won't do; but what about evolution-with-a-purpose, the purpose being the creation of consciousness, nay even self-consciousness, and man himself being in control of the direction and trend of the higher stages of this self-consciousness? If the universe is really to be viewed in this way then such a view is obviously not materialistic. Man is not an insignificant speck of dust. Deity there is—of a kind at least, even if perhaps amorphous and vague, and even if the relationship between deity and humanity is difficult to define satisfactorily (especially if the former is not yet fully ' there ' !).

During the last forty years a number of philosopher-scientists have tried to develop theories on this sort of general line. The latest is the French Jesuit priest Pierre Teilhard de Chardin (who was also a biologist and palaeontologist of some fame) who sets forth his views on this theme in *The Phenomenon of Man.*[1]

This book has been widely reviewed and widely discussed since its publication. All are agreed that it is tough reading,[2] in fact *a difficult and obscure book.* This is due perhaps partly to the style in which it is written. It is often ' poetical ' or ' mystical ' (i.e. life emerging from matter ' is dripping with molecularity ' (p. 92), or ' All round us one by one like a continual exhalation, " souls " break away, carrying upwards their incommunicable load of consciousness ' (! !) p. 272)) and this seems hardly excusable when we learn that the author opens his Preface thus: ' If this book is to be properly understood, it must be read not as a work on metaphysics, still less as a sort of theological essay, but purely and simply as a scientific treatise'. But indeed it is difficult to see how we *could* read it purely and simply as science, for it contains also philosophy and theology. One result of this is that even competent readers are far from being unanimous in their endeavours to explain what the book is about. Some have lauded it to the skies; others have dismissed it as utter charlatanism. But in any event the book is *important* if only because of the great interest it has aroused in quite large sections of the thoughtful public. Should the work be basically pseudo, it is *important* that its fallaciousness should be exposed.

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[1] Pierre Teilhard de Chardin, *The Phenomenon of Man* (translated by Bernard Wall), with an Introduction by Sir Julian Huxley. Collins, London, 1959. Pp. 320. 25s.

[2] The present reviewer must confess that had it not been for Sir Julian's so lucid ' Introduction ', he would have found the ' tough reading ' still tougher!

2 *Outline of Teilhard's Theme*

Mankind in all its aspects is a phenomenon to be investigated scientifically in the same ways as all other phenomena, so that human history like all other history and all human values are proper objects for methodical study. Man is thus not to be regarded as standing outside nature. It is not enough to conceive of him as merely evolved *out of* nature. His is an evolution which always, and all the way through, presents him as united *in* nature. He and the whole universe of which he is part-and-parcel is one enormous process of *becoming*, and in its development reaching higher and higher reaches of being, organisation, and complexity.

Teilhard recognises two different kinds of energy, (1) tangential and (2) radial. The former is more elementary; it is mechanical. Any increase in this form of energy retains its unity as a mere addition of components so that any aggregate reached remains at the same level of organisation as the separate components. But (2) radial energy, in any accumulation of its components, rises to more advanced levels of organisation and complexity so that its unifying centre becomes ever 'higher' in relation to the basic level of its components. The evolution of nature, from elementary particles culminating at last in the manifestation of consciousness is by the continuous growth of radial energy, thus we have a non-stop process of 'becoming'.

All this is a complete departure from the Aristotelian tradition of a 'fixed' universe. In fact Teilhard tells us, more than once, that we ought not any more to use the term 'cosmology' but rather *cosmogenesis*. Things cannot be classified any longer into a limited number of fairly clearly defined species. Instead of a steady-state universe Teilhard presents us with one that in all its parts is continually striving to be something different (and higher) at every moment.[1]

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[1] It is worth reminding ourselves however that at least this part of the Abbé's doctrine (this anti-Aristotelian conception), in essentials, is not *new*. Others have said much the same thing, notably Sir Charles Sherrington in *Man on his nature* (Cambridge 1940). And indeed there were elementary glimmerings in Heraclitus—500 B.C.

One very important thought in Teilhard's treatment of this evolution or becoming is that nothing at any stage of development, not even in the inorganic, comes (as it were) as 'a bolt out of the blue'. Everything has always already existed at an earlier stage if only in an obscure and primordial way. Love, for example, is not a new phenomenon coming to birth with the arrival of man. We can find a kind of premonition of love even in the presentient world (even the inorganic) in the propensity of molecules to unite; for example, sodium chloride is born out of the love of sodium for chlorine! So you cannot say 'life began here', or 'at this point', for life was there from the start. And even 'from the start' is a false expression for there was not anything that could properly be called a 'start'.

So to describe man properly we must go back in history, to his origins, nay even to his pre-origins, and even here we have to look for obscure and primordial precursors (*ad infinitum*).

But this is not all. We have to evaluate forwards as well as backwards, the future as well as the past. Man, as well as other phenomena, can not be evaluated or satisfactorily described simply in terms of his origin; he must be viewed also in the light of his evolving direction, his inherent possibilities and limitations, and his calculable future trends. Thus total man involves a complex space-time relationship. He is what-he-has-been and what-he-is-becoming in relation to a 'becoming' universe of which he is the present apex. When man evolves far enough by reaching higher levels of mental, moral, and social organisation, through the build-up of radial energy, Teilhard implies that a new word will have to be invented (superman?). The gradual ascent of man from the brute to higher and higher levels of humanity is called by the author, a process of *hominisation.*

Complexification. How does this great chain of evolution operate? By means of an ever-increasing complexity. Teilhard calls this whole process *complexification.* Cosmogenesis involves increasingly elaborate *organisation* which in fact we find as we pass from subatoms to atoms, from atoms to inorganic molecules and then on to organic (molecules), to sub-cellular living units, then to cells, many-celled individuals, cephalised metazoa with brains, then to primitive man and finally to civilised (and eventually to super-civilised!) societies.

But in addition to this (simple!?) form of *complexification,* there also occurs at all evolutionary levels a tendency towards *self-complexification*—this seems to mean tendency to higher degrees of the organic accompanied by more and more self-reference (i.e. *enroulement organique sur soi-même*). Thus in human beings (because of this tendency) evolution is apt to produce more and more mental convergence going hand-in-hand with a growth in self-consciousness.

Divergence and convergence. But this reference to *convergence,* and to *self-consciousness,* is perhaps to anticipate unduly. Let us go back, take cosmic evolution in its broad span, and its totality, and ask whether (according to Teilhard), and in what way, evolution makes for convergence, divergence, or both?

Through a period of some two thousand million years up to the advent of man evolution has been *genetical* having a natural tendency to produce separation and division; by a sort of centrifugal differentiation the cosmos has developed into more and more fragmentation. But with the advent of man evolution becomes convergent, through the common mental activity of humanity mediating through social, cultural, spiritual and aesthetic movements, there develops an overwhelming progress towards ever greater *unity*. This is a new style of evolution (pp. 166 and 286), quite different in its nature from the genetic. It is a sort of *psychosocial* evolution made possible by the fact that man, and man alone, is the only species that has developed a reflective consciousness—one capable of thinking of *itself as also being a phenomenon* among other phenomena. The development of self-consciousness tends more and more to superimpose centripetal or centrifugal tendencies. So the long history of cosmogenesis and hominisation with its divergence followed by convergence may be illustrated perhaps by the meridians on a globe that begin by diverging as they leave the Pole but which approach one another again after the Equator . . . (p. 272).

The parallel of course must not be understood too exactly. It is not literally accurate, for the psychosocial evolution of man is not solely centripetal or convergent. Teilhard fully recognises that as civilisation develops there are divergent trends within the total sphere of the convergent. There is, for example, cultural differentiation, that is the production of numbers of psychosocial units with different cultures. But these are not strong enough or lasting enough to disintegrate the predominating urge towards unity. Cultural diffusion, fostered by migration and improved communications, tends to lead in time to cultural convergence.

As this evolution towards greater convergence develops through psychosocial media, the mental activity of mankind becomes more and more complex. This mental activity, or sphere of mind, the author calls the *noosphere*. All the separate parts of this noosphere, as a result of easier world communications and the increase in populations, are being gradually fused together, so that there is an increase of tension and complexity of organisation. This kind of feverishly congested psychological, or psychosocial, activity is pushing (or perhaps ' leading '?) humanity to ever higher phases of hominisation.

These higher phases Teilhard calls ultra-hominisation—the gradual growth of mankind into one all-embracing psychosocial unit. Evolution has already advanced so far in the way of producing a sort of unifying centre to mankind's system of thought that this may reasonably, he thinks, be conceived of as a ' head ' (a sort of cephalisation of society). This he sees as an analogy with the literal and physical fact that the human (and other) organisms have evolved heads as the centrally directing part of the body. Teilhard believes that, as psychosocial evolution advances, this head of human society will come to recognise itself and understand itself more clearly, and that thus our descendants will be enabled to plan and control the future trend(s) of their evolution more completely.

Omega Point. But the Abbé has even more than this in mind. The end is what? Even that higher phase of evolution, self-determined hominisation, is not an end in itself. It must (apparently inevitably?) lead to *point Omega.* The purpose of evolution is the production of consciousness in ever higher and more intense form. Not only hominisation but all cosmogenesis is directed to that end. Point Omega is the final state of all this gigantic becoming—it is a kind of super-consciousness, or rather Super-selfconsciousness. The main argument for our belief in the reality of Omega is simply that since life, and even consciousness, has had precursors or ' premonitions ' in the past, even at elementary levels, the very law of this evolution compels us to believe in the attainment of higher levels of consciousness in the future.

Two main things are contributing to the achievement of Omega, according to the Abbé. The first is our growing knowledge about the Universe generally (including outer space, and also humanity both as individuals and as society). The second is the intensification of feverish psychosocial energy on a planet of limited surface thus making for more and more unification in human life and thought (pp. 250-260).

What is Omega point? Teilhard says[1] it is a distinct centre radiating at the core of a system of centres, and that it is a Soul of souls, and also that it is autonomous, actual, irreversible and transcendent. From one point of view Omega is *human society* the individual members of which have become so united in mind and heart that they have, as it were, got a single intellect and will and set of attitudes between them all. It is human society having evolved into a Hyper-Person.

From another point of view Omega is simply God or Deity. But in some way that Teilhard does not make clear, God and Society are quite distinct. God is not identical with humanity. There is not pantheism (p. 309). In this omega point all man's conflicts are resolved in God, for God is the reconciling principle of Unity. And yet God is transcendent; humanity is not simply swamped by, and merged in, God. At the Omega Point, he says we find ' the Future-Universal '. Future! And yet it is also ' in some sort loving and lovable *at this very moment* '!

But Teilhard is not content to assert that Cosmogenesis and Hominisation (which are essentially what the universe is) involve belief in some form of theism. It is according to the Abbé specifically Christian. That is to say the God of the Universe is specifically and necessarily the God of the Christians. 'God (is) the Centre of centres. In that final vision the Christian dogma culminates' (p. 294). And again,

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[1] On pp. 262, 268, and 271.

> To live and develop the Christian outlook needs an atmosphere of greatness and of connecting links. The bigger the world becomes and the more organic become its internal connections, the more will the perspectives of the Incarnation triumph. That is what believers are beginning, much to their surprise, to find out. Though frightened for a moment by evolution, the Christian now perceives that what it offers him is nothing but a magnificent means of feeling more at one with God and of giving himself more to him. In a pluralistic and static Nature, the universal domination of Christ could, strictly speaking, still be regarded as an extrinsic and super-imposed power. In a spiritually converging world this 'Christic' energy acquires an urgency and intensity of another order altogether. If the world is convergent and if Christ occupies its centre, then the Christogenesis of St. Paul and St. John is nothing else and nothing less than the extension, both awaited and unhoped for, of that noogenesis in which cosmogenesis—as regards our experience—culminates. Christ invests himself organically with the very majesty of his creation. And it is in no way metaphorical to say that man finds himself capable of experiencing and discovering his God in the whole length, breadth and depth of the world in movement. To be able to say literally to God that one loves him, not only with all one's body, all one's heart and all one's soul, but with every fibre of the unifying universe . . . that is a prayer that can only be made in space-time (pp. 296-297).

So the progress of the sciences reveals a Christogenesis. Christ is becoming, but also He *is*, and He *was*; so mankind is reassured.

(I can only hope that the above has been, on the whole, a fair presentation of Teilhard's main thoughts. But it must now be examined in order to discover how valid, or otherwise, it may be.)

3 *Critical Evaluation*

In trying to make a critical assessment of this book, it is desirable to divide it into two parts. (*a*) The first and larger part (but doubtless not in the author's view the more important) is, in the main, straightforwardly scientific and factual, being largely palaeontological and biological. (*b*) The later chapters are much less empirical and far more 'mystical' and speculative, relying considerably on theological, and quasi-theological concepts, and these tend inevitably to be suspect from the point of view of strict science as being usually somewhat gratuitous.

Let us start with (*a*) (above). The general thesis that the universe, with man at its apex, is evolving to higher and higher stages of development and organisation, that man is unified *in* nature as a phenomenon sharing in the common being of all other phenomena, that evolution manifests both divergent and convergent traits, and that man can only be properly evaluated in the light of his future development as well as in the light of his past—all this seems scientifically respectable, and few educated people these days would wish to dispute it.

And indeed further, when he claims that evolution, with the appearance of man and his power of *reflection*, has entered into ' an entirely new field of evolution ' (p. 286) (what earlier in this article I have called *psychosocial* evolution), he is simply saying something with which most scientists would agree. The thought is momentous and valuable.

But when we are told (pp. 243 and 258) that evolution is simply equivalent to *rise of consciousness*, and even increased consciousness, one cannot but wonder whether Teilhard is not shutting his eyes to some inconvenient facts. Admittedly evolution *generally* tends towards consciousness and more consciousness. But not always. Bony fishes and birds have not developed cerebralisation and yet they are successful lines of evolution, i.e. after the lapse of a relatively long period of time these species continue to exist and to flourish. Thus the *unity* of the universe (which Teilhard so earnestly seeks) is impaired.

And again, believing that evolution is *essentially* leading to consciousness, he stresses that cosmogenesis has a definite *direction*. But has it? Most lines of evolution lead to the complexification of the nervous system (necessary for consciousness), but some do not.

(*b*) *The mystical and speculative parts of the book—being in the main concerned with ' point Omega '*. If we look upon Omega as a general conception according to which more perfect *hominisation* looms on the *possible* horizon of man's future, it may seem reasonable enough and worth our belief. But Teilhard comprehends much more than this. Bearing in mind that he writes more often than not of ' Omega Point ' and not just ' Omega ', we must ask the following pertinent questions:

(1) Have we good and sufficient reason to believe in the *reality*[1] of Omega point?

(2) Is Omega point (Higher Consciousness, God) which is (being?) produced by Evolution consistent with the Christian concept of God?

(3) Is Teilhard consistent with himself in the account which he gives of the nature of Omega Point?

(4) What is the relationship between Teilhard's God and the Ultimate Hyper-personal Society, and does such relationship seem reasonable?

(1) *Is Omega logically demonstrated?* Teilhard has clearly warned us that his book is to be read ' purely and simply as a scientific treatise ' (p. 29). Also, he has stressed that he arrived at his Omega point by a remorseless logic. In fact this concept is not scientifically demonstrated at all. It is

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[1] i.e. not as Something that may be *possible* in time, but as Something actual and even APODEICTIC.

simply asserted. 'To be supremely attractive, Omega must be supremely present'! Of what kind is the argument here? At least it is not the argument of science. And indeed does he not betray the wishful thinker and 'give the show away' when he writes '. . . *doubtless I should never have ventured to envisage the latter* (i.e. Omega Point) *or formulate the hypothesis rationally if, in my consciousness as a believer* (i.e. in the Christian dogma), *I had not found not only its speculative model but also its living reality*' (p. 294, italics mine).

And if the sciences, on their own account and unaided by Faith, were unable to afford this scholar with his undoubtedly scientific mind good and sufficient reason to believe in the reality of Omega Point, are his readers likely to find that same *a priori* Faith unnecessary in order to accept the reality of Omega? Surely not.

(2) *Is Omega consistent with the Christian's God?* In his exposition of Omega Point, Teilhard clearly seems to have allowed (surely he must have perceived it?) a confusion of thought between God-in-the-past and God-in-the-Future, between the Christian God and an Emergent Deity. In fact, Teilhard by making no explicit recognition of the truth that these two conceptions (at least *prima facie*) are the contrary, the one of the other, gives the impression that they are one and the same. It is as if (for the Abbé) the concept of Deity could not be *in any sense* related to the *time series*. But this surely needs searching examination? The question might be asked thus:

Is God (*a*) *before*, or is God (*b*) *after*, Evolution? (And if both, How so?)

(*a*) Traditional (Orthodox) Christianity depends basically on the assumption that God, perfect and fully-existing, was 'there' prior to (BEFORE) the beginning of things.

(*b*) But (as opposed to this) lately the Emergent Evolutionists (Samuel Alexander and the like)[1] have gone to the opposite extreme, and have 'preached' God as emerging—as striving towards fully realised existence only in the higher reaches of mind-in-evolution. According to this view God is (as it were) AFTER evolution.

Now Teilhard—who regards his book as primarily a kind of Vindication of Christianity—where does he stand in this polarity? Is his Omega point in line with (*a*) (above)? The answer is clearly that much of his treatment of Omega is quite *inconsistent* with the God of orthodox Christianity. For example (on pp. 268-9) he says 'the idea that some Soul of souls should be developing at the summit of the world is not as strange as might be thought . . .' *Developing* indeed! (Is not that an *Emergent* Deity . . . or near enough?) And there are other passages (pp. 259–60) that strongly suggest that Teilhard's Deity is still only evolving-towards-Himself! Anything but Christian!

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[1] Teilhard does not mention any of them in his book, not Alexander or Lloyd Morgan, or even Bergson (who had definitely influenced him in his youth). But unquestionably much of the so-called 'Emergent Evolution' as understood in the 'twenties is easily recognisable in this book. Dare one ask—'can it possibly have been that Alexander and Company had more " nerve " than Teilhard, that they did not need the comfort of Christian myths?'

(3) *Is Teilhard's account of Omega self-consistent?* But it is apparent that the inconsistency just noted above between Omega and the Christian Deity falls back upon something deeper, namely, a fundamental inconsistency in Teilhard's treatment of Omega itself. He surely contradicts himself? For instead of writing consistently of Omega which is (in process of) coming-into-being, he says on page 291 of his book (as if creating a bulwark for Orthodox Christianity) 'Omega is . . . *already in existence*'. (And, incidently, one may wonder why the italics?).

Does it not seem as if neither science nor Orthodox Christianity could be satisfied with this book, as it stands? The Deity has not been shown to be 'Christian-And-Emergent', nor yet 'Either Christian or Emergent'. There is no Deity at all. He 'falls between two stools'.[1]

(4) *What is the relationship between Teilhard's Deity and the Ultimate Hyper-personal Society?* We are evolving (all of us together) into an ultimate hyper-personal society; 'through science and the philosophies' we advance to 'a collective human *Weltanschauung*' and in this we have the first symptoms . . . of a still higher order . . . somewhere ahead a point which we might call Omega . . .' (p. 259). This looks like pantheism. But Teilhard strongly refutes such a notion for, he says, this 'universal centre of unification . . . must be conceived as pre-existing and transcendent' (p. 309). Or, he says, call it 'pantheism' if you like but it is 'an absolutely legitimate pantheism' (p. 310).

But surely, and at best, these are only 'pious' assertions that contradictions are resolved, not *demonstrations* as to how we can satisfactorily conceive of the relationship between an Omega that is a Super-social Organism and an Omega that is a pre-existing transcendent Deity?

4 *Summing-up*

To sum up. *The Phenomenon of Man* can lay claim to a modicum of importance and is sound enough in so far as it does supply a considerable amount of concrete (scientific) evidence for the belief that life in general, and man in particular, are evolving towards higher levels of being which may be more and more realised in the future.

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[1] Nevertheless it must be admitted in fairness to Teilhard that it is only from the point of view of *traditional modes of thought* that he can be indicted for a *prima facie* inconsistency, for sometimes writing as if God were only in process of emerging, and at others as if God (as the Bible teaches) was in full possession of his Deity at the beginning. For it seems just *possible*, if one could rise to a plane of thought based on some *higher synthesis* (as, for example, by the application of Einstein's Relativity principles) that the idea of Perfect Deity gradually-evolving is still consistent with Perfect-Deity-at-the-beginning as the presupposition of the Universe, but *prima facie*, so ridiculous is this that it certainly cannot be taken for granted, or the problem of reconciliation ignored, as it seems to be in the book under review.

In general terms, the book is a reassertion of belief in human (as well as cosmic) progress so dear (though of narrower interpretation) to the nineteenth century, and so suspect and unfashionable, in many quarters, in the twentieth. Belief in man. This is a tonic that mankind sorely needs in these days of spiritual doubt and social change. And when belief is based on the demonstrations of scientists rather than on the wistful yearnings of uncritical prelates (as has been the lot of mankind till quite recently), then there is sound hope for man's future.

And in this note of optimism and positivism, happily for our age, this work does not stand alone. It is in line with the thoughtful optimism of Professor Michael Polanyi's 'Beyond Nihilism' (Eddington Memorial Lecture 1960).

But it is difficult to see how one can go further along with Teilhard, how one can accept his contention that his 'Future Universal' (Omega) which the sciences do indeed seem to be *presaging*[1] is to be equated with the traditional transcendent God of Religion and (as if this was not asking enough of the sciences) even of vindicating the Christian Faith. Here there is surely a saltus, a *Deus ex machina.*

If this criticism is valid, then the book under review illustrates very well how greatly the tradition of much Western philosophy has been vitiated by the tendentious thinking of theologians, or at least by theologically-minded philosophers; vitiated by men who, nurtured in the first place by some fortuitous set of religious dogmas, set about (consciously or subconsciously) to rationalise them, to find a theory of the universe which would vindicate a specific[2] religious tradition. Let us hope the universe may be 'proved' teleological, let us even hope (if we want to) that Deity, in some sense, may be *proved*, but in the name of truth let all our philosophy be scientific (i.e. objective, unprejudiced, disinterested, the comprehensive product of cosmopolitan mind) and not the result of our finding fortuitous reasons for what we believe because of our personal instincts, education, or particular cultural tradition. (The universe is too big and too complex for such petty-minded philosophising).

Finally, the fact that *The Phenomenon of Man* has been more of a 'hit' than it deserves, that it has received exaggerated acclaim from so many Critics is easily explained. There is in many quarters today a climate of dread uncertainty in matters of religious faith, a spiritual loss of nerve, a explanation and theory-construction. For him such ideas are not only psychological devices which may cause the production of important laws. They also play an essential rôle in the *arguments* which lead up to such laws and theories as well as in the conditions for a satisfactory explanation.

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[1] The Abbé would no doubt reject 'presaging' here as not being strong enough. His word (in English), one imagines, would be 'proving'.

[2] This is not only exemplified in the 'Scholastic' philosophers. We find it too in Hegel, and in this country in most of our Gifford Lecturers even in the past few decades.

Before discussing the possible applications of this interesting and fruitful point of view it would seem to be advisable to free it from a restriction that is not really necessary. The restriction is that the model, or the analogue used is required to be familiar (32) and not purely formal (159). Considering that the author makes understanding a necessary criterion of a satisfactory explanation this restriction would seem to be quite plausible. However it is well known that intuitive plausibility is very often the result rather than the presupposition of the continued application to concrete problems. It is therefore not advisable to exclude a model, an analogy, or a more general idea on account of its initial unfamiliarity. A second criticism is much more important. The criticism is that the author has failed fully to utilise the power of his theory of explanation. He has not applied it to other items dealt with in his book and he has thereby missed the opportunity of giving the book a more coherent appearance. Thus his discussion of the peculiarities of scientific description would have greatly gained from relating it to the context of explanation and theory-construction. First of all the notion of an observable could have been made much clearer in this way. Observables are introduced as elements of the smallest possible set of predicates capable of specifying all the properties of a system (48). This definition produces a well defined set of observables only if the set of properties of the system investigated is also well defined. The only way of obtaining a well defined set of properties is by appeal to the theories we possess. But if we are allowed to appeal to them, then we may as well define the notion of an observable in a more direct way, namely as referring to all the primitive descriptive terms occurring in these theories. Secondly, the syntactical means for forming scientific descriptions are of course also dependent on the theories and should be explained in connection with them. (By the way I do not think it to be the task of a philosopher of science to popularise such notions as the notion of a function, of a functional and other mathematical notions; this applies even to introductory courses such as the one under review. My advice would be either completely to omit technicalities—and there is quite a lot to be discussed in a non-technical manner—or to assume them as known. Too much of contemporary philosophy of science is nothing but bad, because unnecessarily technical, popularisation!) Thirdly both the explication and the reduction of predicates (65) is understandable only if we discuss the conditions according to which theories in a certain domain are replaced by better theories in the same domain. For example the replacement of such qualitative predicates as pitch, loudness, timbre by quantitative predicates (frequency, amplitude, intensity of Fourier components) can be justified only by pointing out that the new predicates play an important rôle within theories which, judged by the standards of explanation, are more powerful, more satisfactory, more general than the (already very powerful) theories utilising the qualitative notions (the theory of harmony is an example). Hence also reduction and explication must be discussed in the context of explanation and becomes quite unintelligible when separated from this context. It should of course be mentioned, in all fairness to the author that this method of gradually proceeding from the less complex (concepts, statements) to the more complex (theories, sets of theories such as occur in explanations) is quite customary in contemporary discussion of scientific method. To a certain extent it occurs even in such an excellent treatise as Professor Nagel's recent *Structure of Science*. However, just as a concept can be made clear only from the function it plays in a statement—a fact that has been seen, among others by Wittgenstein who therefore in his *Tractatus* took sentences as the fundamental units of discourse—in the very same manner a sentence, its structure, the way it will be replaced, in the course of scientific progress, by a different sentence can be made clear only by pointing to the rôle it plays in the wider context of a theory, or of an explanation, that last and final unit of scientific knowledge. It seems to me that many of the more technical problems that at present beset philosophers of science (examples: What is a scientific law? How can the adoption of vacuously true statements be justified? What rôle do counterfactuals play in the sciences?) will be accessible to immediate solution once it is remembered that all scientific statements must be capable of functioning in explanations (otherwise they would be redundant) *and that all these statements are therefore subject to the conditions of explanation.* That is, the solution of the problems mentioned above will follow from these conditions and it will not of course be available to those philosophers who discuss the status of scientific laws and of scientific description independently of the nature of explanation.

Instead of relating statements to the context of scientific explanation where *reasons* are given for whatever rules (syntactical or semantical) are being used, many philosophers who feel the necessity of relating things to a wider context proceed *historically*. The same is done by our present author. He believes that ' if we want to understand the special sort of describing and explaining that scientists undertake we must first understand the way we ordinarily describe and explain events ' (4). The result of this belief is disastrous. Apart from giving rise to some quite childish analyses of the common idiom (cf. the turtle-dialogue on p. 9) it precipitates discussion reminiscent of the famous explanation of the principles of radio-telegraphy: *Question:* how does radio work? *Answer:* It is quite simple. Imagine a dog extending from Oxford to Cambridge. If you pinch the dog's tail in Oxford, he will naturally bark in Cambridge. Now radio is exactly like this, only without the dog. What I mean to say is that scientific explanation also in the sense of Harré contains so many modifications of what is regarded as a satisfactory explanation within the common idiom that any attempt to explain it with the help of the principles underlying the latter must sound like this little story. I really wish that writers in the beautiful and exciting field of the philosophy of science would stop appealing to the reason of ignorance even if appeal to such reason should happen to be very fashionable.

A similar remark applies to chapter V. This chapter contains some very clearly formulated criticisms of attempts to solve the so-called problem of induction. I especially liked the argument (which is by no means original, but still worth making, especially when it can be presented in a simple and concise form) that the probabilistic accounts of induction are not a bit better off than their less sophisticated predecessors (122). The ' method ' (125) of Strawson and Edwards, too, is rejected by a criticism which is as brief as it is sound and which to me at least seems to go to the heart of the matter (it deals with the *normative* aspects of scientific method (127)). But then the author starts wavering, repeats the now fashionable *bon mot* that scientific procedures ' form a spectrum ' (130) whose elements must be found out by ' a study, from a logical point of view, of the methods and criteria actually in use ' (129) and that it would be a ' fundamental misconception ' to believe that there is a standard way in which theories are abstracted out of factual evidence (129). Now it is of course quite correct to say that there exists a great variety of ways in which theories are first discovered. However if the author had read a little more carefully Professor Popper's book on the *Logic of Scientific Discovery* (to which he refers briefly on page 132 but whose views he regards as ' too extreme to account for many scientific activities ' (133)—as if Popper had intended to give such a *factual* account) he would have realised that this does not mean that there exist equally many solutions of the problem of induction. For example the fact that sometimes generalisations are not obtained by a stroke of genius but in a rather mechanical way (141) does not mean that induction *does* work in some cases. A closer inspection of the allegedly mechanical procedures will always reveal that what we are dealing with are in fact *deductions* from premises which have not been explicitly formulated and which have been obtained by a bold act of imagination in the first place. (This feature of theorising has misled other thinkers, too. An example are the various attempts to establish a ' Logic of Discovery ' including the most recent one by Professor Hanson.) The inductivistic bias of the author is shown also in other places; for example it is shown in his belief (153) that ' generalisations and theories are built up in such a way that they always stand a good chance of being confirmed, a better chance of confirmation than of disconfirmation ' and in the complementary remark that generalisations can hardly ever be shown to be completely wrong—unless ' we begin to apply the law to cases further and further removed from those cases upon which it was [originally] based' (154). The trouble with such a principle of caution is that it is inapplicable unless we regard the law as nothing but a summary of past experiences. It is inapplicable because we hardly ever know (in advance of introducing new theories, that is) what cases are 'close' enough to our initial evidence not to endanger the theory. Considering that Newton's theory evolved from the analysis of planetary motion one would be inclined to think that its application to double stars was very far removed indeed from the original evidence. However it was not through *these* cases that the theory was disconfirmed (or *could* be disconfirmed) but through the much 'closer' case of Mercury. It would be unjust to omit mentioning those instances where the author has definitely abandoned inductivism such as when he says that scientists 'are often prepared to sacrifice something of precision and much of comprehensiveness if some system and order can be imported into a set of generalisations' (72) or some metaphysical idea superimposed upon them (contrast this, however, with 'scientists are the misers of logic; they throw nothing away' (157)). And it would be equally unjust to overlook the use he makes of his idea of explanation in chapters V ('Inductive Reasoning') and VI ('Fitting the Facts'). But the lack of a clear conception of scientific method (saying that there is a 'spectrum' (130) of procedures cannot be regarded as indication of such a clear conception as we know in advance that all sorts of things occur in any kind of activity!), the inconsistencies encountered in the book as well as the ballast of fashionable but otherwise irrelevant ideas it contains make it very difficult to recommend it as what it is supposed to be, namely, 'a short and systematic [*sic*!] account of the workings of the sciences for those students who are taking Logic or History of Science as subsidiary subjects' (i). Whatever valuable elements the book contains could have been condensed (and as a matter of fact had already been condensed by the author (ii)) in an article or two. I really do not see any point in increasing the flood of books on the philosophy of science if twenty pages can do the trick.

I shall conclude with a few scattered remarks: (1) Considering the work of Duhem, A. Maier, Clagett, and others it is high time to stop criticising the Middle Ages for their alleged lack of scientific development (68). (2) It is also incorrect to say that science and the methods characteristic for this enterprise cannot start before a reasonable amount of empirical generalisations has been assembled (43). As has been shown by Popper and Matson and as had already been insinuated by Burnet theoretical physics starts with the Presocratics. It is statements like the one made by the author that have seduced people with insufficient historical information such as the late Professor Austin into thinking that philosophy can be advanced only in a piecemeal manner. (3) The author adopts the position of Mill, Ryle, and Toulmin according to which scientific laws are to be regarded as rules of deduction rather than as premisses (80 f.). Apart from some unenlightening verbal tricks the only argument the author can present in favour of this position is Carroll's infinite regress. That this argument is completely ineffective can be easily seen if we assume that all P-rules (and there will be a finite number of them) have already been turned into premisses. There not being any P-rules left the argument cannot even start. Hence, it cannot prevent this situation either in which all P-rules are used as premisses. (4) A heavy object hitting the sun would not be 'reflected violently back' (144). It would be swallowed by the sun. This remark may seem pedantic but I believe that even a *philosopher* of science has some duty to use realistic examples in order to make the point he wants to make.

P. K. Feyerabend

The Edge of Objectivity. An Essay in the History of Scientific Ideas. By Charles Coulston Gillispie.
Princeton (Princeton University Press); London, Oxford University Press, 1960. Pp. viii + 562. 42s.

The Making of Modern Science. Six Essays. By Gerd Buchdahl, M. A. Hoskin, A. Rupert Hall, Marie Boas Hall, Sam Lilley and Charles Raven.
Leicester, Leicester University Press, 1960. Pp. 56. 6s.

Since the assumptions and conclusions of the philosophy of science are not completely divorced from the practice of science, philosophers of science have the problem: how to learn and understand the parts of science relevant to their enquiry? *Nature* and *The New Scientist* are not really adequate. If one is lucky, one may resuscitate memories of the Mathematical Tripos, and bring them into focus with the lenses of, say, Russell and Braithwaite. Or one may try a brain-picking session with a working scientist, but this produces frustration: the problems usually least interesting to him are those most interesting to the philosopher, and vice versa.

So the enquiring philosopher turns to the history of science. But new problems arise: which history? There is a growing awareness that the last word on the history of mechanics was not said by Mach. Even Koestler's *Sleepwalkers* has not received unanimous acclaim from the professionals. Worse yet, so much of the history of science has been done philosophically. The simple question, 'what was Galileo doing in his inclined plane experiment?' has been answered, not once but too many times. Quite different answers have come from historians with a prior commitment to mathematical idealism, inductivism, refutationism, or dialectical materialism. At this point the philosopher may ask, 'Is there not a general account of, say, the Scientific Revolution, which is reliable but not pedantic, and neither naive nor axe-grinding in its philosophical commitment?' In short, something he can use. It is with this question in mind that I discuss the two books under review. But first I should say that the situation is not desperate; the books on the Scientific Revolution by Wolf (revised by McKie), Butterfield, Crombie, and Hall all go some distance, each in its own way, to meeting the specifications set out above.

It would be hard to find a pair of books on roughly the same subject, with more differences between them. *The Making of Modern Science* is a solid, workmanlike job of popularisation, originally done for the BBC and now printed in this modest booklet. There is a certain unity about its theme, but the separate essays approach their material in different ways, and we have the vociferous outsider, Canon Raven, concluding the book by denying all that went before. *The Edge of Objectivity* is written by an American historian of science justly distinguished for his scholarship and insight. It is a big (and not inexpensive) book, devoted to exploiting one single theme, and doing it throughout with charm and elegance.

Since failure is more instructive than success, I shall concentrate first on Gillispie's book. What went wrong here? It would seem to be a result of his re-writing the history of modern science to prove a not entirely correct and not altogether clear point about the development of science: that it advances in each sphere by improving its 'objectivity'. Each dramatic step forward is interpreted as an advance in the 'cutting edge of objectivity'. There is, of course, *prima facie* evidence for this: the exorcism of spirits (of all sorts) from the natural world was one of the distinctive achievements of the scientific revolution. But 'objectivity' is, without further definition, an omnibus word, and when it is used to describe everything from Kepler's *vis motrix* to Einstein's space-time continuum, it loses what precision it might have had originally.

Also, this cult of objectivity turns Gillispie's essay into Whig history with a vengeance. In this success story there is no mention of those who tried to advance 'objectivity' before the time was ripe, ranging from the Mertonians of the fourteenth century, through the iatromechanists of the seventeenth, to the eugenicists of the nineteenth. One would not wish the history of science to turn into a morbid study of failures, but this succession of miraculous interventions by great 'objectifiers' falsifies what Gillispie himself knows of the character of scientific progress.

Perhaps it was this straining after an impossible moral which caused Gillispie to make a distressingly large number of slips on historical fact. An erratum page lists some, and A. R. Hall's review in *Isis* (September 1959) gives a goodly number more. An example of a more serious error, missed by both these, is Gillispie's ascription of Fermat's 'minimum principle' theory of refraction to Descartes. The difference was not one of detail: the Fermat theory yielded a reduced velocity of light in a refracting medium, and the Descartes theory an increased velocity. When an historian finds this sort of error appearing within his own field of competence, he is reluctant to rely on the book outside this field, and its value as a scholarly contribution is seriously reduced. Even on topics closer to his special interest, Gillispie makes unfortunate slips. Thus it is not correct to say:—'Diderot, ever the master of the philosophic fiction, created a blind mathematician, one Saunderson, who could conceive geometry to perfection. He had a post at Cambridge. But he had only a partial grasp of the real world . . .' (p. 164). If this blind Saunderson had a creator, it was not Diderot, but *le bon Dieu*; he succeeded Whiston to the Lucasian Professorship in 1711, and was a well-known figure in English science (D. E. Smith, *History of Mathematics*, vol. I, p. 454, portrait p. 455).

One clue to the failure is in the Epilogue. There one turns, hoping for a drawing-together of the threads of the argument, and a clear statement of the message of the book, which up to then had been relegated to a few sentences at the end of each chapter. What one finds is a highly technical analysis of the different problems leading to Einstein's relativity theory: Mach and Maxwell are fitted into place, and marks for objectivity are awarded. But why is this an Epilogue, rather than a last chapter? And is this the end of the story? Since 1905 there have been a number of theoretical developments with philosophical interest, among them the interpretations of quantum mechanics, turning on this very question of objectivity. On this Gillispie has not a word to say.

Gillispie's failure to state his case cogently suggests that he had difficulties with the case itself. This lends support to my suspicion that this is not 'the book' with which he has been living for some years, but a peculiarly muted version of it. (He mentions at the end that he found it impossible to work within the agreement with the publisher who first commissioned the book, and so transferred it to the Princeton University Press). For the moral of this present story relates only to the intellectual content of scientific explanation, and over the years Gillispie has shown himself to be on to a bigger, and more fundamental, problem. 'Objectivity' is more than a successful tactic; it is a commitment without which science cannot survive. Let me quote a passage, starting (for brevity's sake) with the last line of a quotation from Galileo:

> But the conclusions of Natural Science are true and necessary, and the judgment of men has nothing to do with them.'
>
> To suppose otherwise is to give the game away. It is to agree that the papal court was justified when it decided to condemn Galileo on the ground that the Copernican system, however convenient as a mathematical device, tended to

> unsettle men's minds and weaken authority and order. It is to agree that the Soviet state was justified when it imposed Lysenkoism on the science of genetics as a theory conformable to Marxist dogma.[1]

Now, this is a challenging problem. I recommend it to philosophers. Had Gillispie written his book about that, we would have had something to argue about. But for reasons of his own, he did not, or perhaps could not. And on the problem which seemed historically more straightforward, and philosophically more fundamental, he foundered. It is tragic. Finally, advice to philosophers interested in science: a stimulating book, but *caveat lector.*

To turn from a brilliant failure to a modest success, we consider *The Making of Modern Science.* If shows that Gillispie's hope of winning for history of science ' a place in historiography comparable in interest and professionalism to that which the philosophy of science has long held in philosophy ' is within sight of being realised. We now have a generation of historians who are prepared to recognise and grapple with the complexities of history, and not try to smother them under a single all-embracing philosophical blanket. Historians of different persuasions can now exchange the insights produced by their special viewpoints, and not merely indulge in polemics on the demerits of each others' grand syntheses.

The authors of these brief essays are not concerned to give a potted version of the technical history of science in the century of revolution. They presuppose an adequate knowledge of what actually happened, and consider a much more interesting problem. This is to give a sketch of the history of the concepts, methods and presuppositions of science, an investigation now called ' the history of scientific ideas '. This study is only now getting underway as a discipline, and is perhaps the most difficult of all sorts of histories to pursue. I think that it is a mark of the quality of these essays that the difficulties appear as clearly as they do.

The first two deal with more straightforward problems, and are correspondingly easier. Gerd Buchdahl reviews the explicitly formulated philosophies of science of Bacon and Descartes, and shows clearly that the difference between them was by no means one of hack inductivism against arrogant apriorism. Michael Hoskin, on ' Nature and Mathematics ', restricts his historical range even more severely, and gives a lucid account of Galileo's approach, contrasted with the traditions of Plato and Aristotle. A. Rupert Hall's ' The Experimental Way ' is really philosophy done by example: experiment may precede theory, or it may follow; it necessarily involves abstraction from raw nature, but may be misleading because of this. The examples range through the entire century, and here one feels that Hall has achieved relative clarity at the price of some oversimplification. The matter is not simply one of reasonable alternatives; natural philosophers of the time disagreed with each other, and struggled with their own ideas, in an attempt to find the best approach in any given context. The Descartes-Harvey controversy on the motion of the heart turned on this,[1] and would serve as a useful reminder of the difficulty of extracting Nature's secrets.

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[1] Letter to *American Scientist*, 45 (September 1957), 266A-274A, answering criticisms of his review of Needham's *Science and Civilisation in China*, Vol. II.

With Marie Boas Hall and Sam Lilley we are at the heart of the problem. 'The Machinery of Nature' and 'The Nature of the Physical World' discuss, from different aspects, the astonishingly sudden rise and acceptance of the 'mechanical' view of the world. Lilley is most interested in the extrapolation of this view into the human sciences in the eighteenth century, but he is also concerned with the problem of its origin. This, he feels, cannot be satisfactorily explained by the internal dialectic of late sixteenth-century science; for an external cause he suggests, as a first approximation, the world-view of a commercial society. His problem with this is, of course, chronology: why should it be that the centuries-long decay of the 'organic' Medieval society should suddenly catch the imagination of learned men in the first third of the seventeenth century? Marie Boas Hall sets herself the most difficult task of all: to describe the origins *and* outcome of the revolution in thought, and also to indicate its problems at various stages. Small wonder, then, that she has to fall back on sweeping generalisations to get her story told. Thus we find that 'the rational tendency of thought of the age . . . eagerly welcomed the mechanical view as the antithesis of the magical occultism of preceding ages.' Here is a causal explanation more closely tied to chronology than that of Lilley, but it still lacks precision. One is anxious to know more of this 'rational tendency', and more of the 'magical occultism' which was rejected: there was a world of difference between Fludd and della Porta; and the explicitly stated battle of the new philosophy was not with magic, but with the Aristotelian tradition.

Finally, Canon Raven tells his colleagues that they are barking up the wrong tree. Not astronomy but gastronomy is the fundamental human science, and the biological sciences were the prime movers of the revolution. Moreover, 'the heliocentric cosmology was less disturbing than the rejection of spontaneous generation, of creation as an act rather than as a process, and of witchcraft, astrology and magic.' It could be argued that the latter changes were the result of the former; but although the 'closed world to infinite universe' change is commonly taken as the fundamental one, this priority has not, to my knowledge, been established by historical studies.

Thus this book shows us the history of science at an exciting stage in its development: it is sufficiently far advanced in the creation of a discipline, with a base in agreed fact, to venture into higher-order problems, and discussion of causes. Philosophers, as well as social and cultural historians, will have a contribution to make, for none of this generation of historians lacks respect for philosophy of science as an independent and important discipline. The only danger is that philosophers may misread surveys such as this book, and think that 'little Jack Horner' techniques will advance the history of science; but Gerd Buchdahl has shown that this need not happen, and that the two disciplines can be fruitfully combined. To sum up, this collection of essays serves admirably as a high-level introduction to the history of science at one of its most interesting and rapidly developing points.

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[1] J. A. Passmore, 'William Harvey and the Philosophy of Science' *Australasian Journal of Philosophy*, 1958, **36**, 85–94. Etienne Gilson, *Études sur le rôle de la pensée médiévale dans la formation du système cartésien*, Paris 1930.

J. RAVETZ

The Correspondence of Isaac Newton. Vol. I: 1661-1675; Vol. II: 1676-1687.
Edited by H. W. Turnbull.
Cambridge University Press (for the Royal Society). Vol. I: 1959. Pp. xxxviii + 468. Vol. II: 1960. Pp. xiii + 552. 7 guineas each volume.

Newton with his prism and silent face,
The marble index of a mind forever
Voyaging through strange seas of thought alone . . .

THE familiar lines of Wordsworth on the statue in the antechapel of Trinity College, Cambridge, come to mind with emphasis on the last word, when one reflects that more than two centuries after Newton's death there is still no sign of a collected edition of his works and that there are masses of his manuscripts which have never yet been seriously and systematically studied by competent scholars. From time to time plans have been made, but the sheer magnitude of the task has daunted all concerned. Some years ago it was decided that the most that could be done 'within the next decade' would be to produce a complete and critically annotated edition of Newton's letters. In 1938 Professor H. C. Plummer agreed to undertake the editorship. After his death in 1946, the Newton Letters Committee was reconstructed under the chairmanship of Professor E. N. da C. Andrade, and Professor H. W. Turnbull was appointed editor. Recently, owing to ill-health, Professor Turnbull resigned (and has since died) and the work that remains to be done is in the hands of Dr J. F. Scott.

For the first time we can now get a general idea of Newton's correspondence. Of the 156 letters and documents in volume I, 137 have been published before but not together. Much of this early correspondence was with

Oldenburg and Collins, both of whom often acted on Newton's behalf in his relations with other scientists. Oldenburg was Secretary of the Royal Society until 1677 and kept in touch with scientists on the Continent. Collins was a civil servant in London who maintained a lively correspondence on problems of mathematics. With but two exceptions, all the letters in volume I deal with scientific or mathematical subjects. The first is a short note reproving a friend who was suffering from a hang-over. Its interest lies in the fact that it was written in a form of phonetic spelling of Newton's own devising. The other non-scientific letter, dating from 1669, addressed to Aston, later a Secretary of the Royal Society, gives advice on how to behave and what to observe when travelling abroad. For example, Aston is urged to ask questions rather than make peremptory assertions, 'it being ye design of Travellers to learne not teach'.

The two main subjects of the correspondence in Volume I are optics and mathematics and there is a remarkable contrast between Newton's willingness to impart his views on the former and his reluctance to do so in the case of the latter. Indeed, in one of the most interesting letters in this volume (No. 22), from Collins to James Gregory (in 1670), Collins, describing the only two occasions on which he met Newton and the problems which they discussed, writes with reference to an annuity problem, 'I wrote to him about the same, hoping to have had his Series communicated, with the method of its Invention, but I received no more in answer than what I sent you, hence observing a warinesse in him to impart, or at least an unwillingness to be at the paines of so doing, I desist, and doe not trouble him any more.' But for Newton's secretiveness, the regrettable priority controversy concerning the invention of the calculus need never have arisen.

The correspondence concerning optics deals with two main themes, the reflecting telescope and the composite nature of light. That relating to the latter is of supreme interest because it reveals the vital differences of outlook between Newton and his critics. For example, Huygens (letter no. 92) wrote to Oldenburg about Newton's theory of colours, 'il resteroit la grande difficultè d'expliquer par la physique mechanique en quoi conciste cette diversitè de couleurs'. In vain Newton replied to Oldenburg (letter no. 103), 'I never intended to show wherein consists the nature and difference of colours, but onely to show that *de facto* they are originall and immutable qualities of the rays which exhibit them, and leave it to others to explicate by Mechanicall Hypotheses the nature and difference of those qualities; wch I take to be no very difficult matter.' The clash was one of two different philosophies of science, just as was the later clash over the mechanism of gravitation.

In volume II many of the letters continue to be devoted to mathematics. Outstanding among them is the famous 'Epistola Prior' of 13 June 1676, written by Newton to Oldenburg for transmission to Leibniz. It contains the first mention of the binomial theorem by Newton in his correspondence. Although he gives no proof of the theorem, he discusses several particular applications. Leibniz's comments are given in a memorandum (no. 171). In a further letter by him to Oldenburg (no. 172), dated 17 August 1676, he writes: ' Newton's discoveries are worthy of his genius. . . . His method of obtaining the roots of equations and the areas of figures by means of infinite series is quite different from mine, so that one may wonder at the diversity of paths by which one can reach the same conclusion.' This letter is indeed one of the most intriguing in the whole correspondence. For, Leibniz refers to his method as ' a corollary of a general theory of transformations ' which contains ' the true method of indivisibles as most generally conceived ', and the editor remarks that a full elucidation of Leibniz's account of his method in this letter would take us far towards a solution of the Newton-Leibniz controversy. The most general expression of the method of indivisibles, in the sense intended by Leibniz, occurs in the *Lectiones Geometricae* (1670) of Newton's predecessor in the Lucasian chair, Isaac Barrow, and it is an open question to what extent in his discovery of the calculus Leibniz was indebted to Barrow.

The ' Epistola Posterior ' of 24 October 1676 (Letter no. 188) is also of special interest because in it Newton reveals the steps which led to his early mathematical discoveries. In particular, he seems to have been greatly influenced by the arithmetical works of John Wallis.

In the latter part of volume II we first encounter Newton's ideas on motion and gravitation. In a letter to Boyle dated 28 February 1678/9 (no. 233), he puts forward his hypothesis of a universal aether ' much like air in all respects, but far more subtle ', and discusses it at length. At the end of this letter there is a paragraph in which he sets down ' one conjecture more which came into my mind as I was writing this letter. It is about ye cause of gravity.' He supposes aether to consist of parts of different degrees of subtility and that in the pores of bodies there is much less grosser aether in proportion to finer, than in the case of air and ' open spaces '. The grosser aether being less apt to be lodged in those pores than the finer aether below, it will endeavour to get out and give way to the finer aether below, and this can happen only if the bodies in question descend to make room above them for the grosser aether to escape into from them.

The most momentous correspondence in this volume begins with a letter (no. 235) from Hooke to Newton, written on 24 November 1679. In his capacity as Secretary of the Royal Society (in succession to Oldenburg) Hooke invited Newton to communicate a paper to the Society and mentions various recent discoveries in astronomy which had been made in Paris. In his reply of 28 November (no. 236) Newton communicates a suggestion for demonstrating the diurnal rotation of the Earth. Although the subsequent correspondence with Hooke soon exacerbated Newton, it had momentous consequences, for in a letter to Halley of 14 July 1686 (no. 290) he revealed that Hooke's letters led him to investigate central orbits with the result that during the winter 1679/80 he first proved that under the inverse square law the orbit could be an ellipse.

Much of the later correspondence in volume II is with Flamsteed and Halley, and relates in one way and another to the writing and production of the *Principia*. But there are also letters which bear on Newton's many other interests and activities. For example, he gave detailed advice concerning the special mathematical foundation established by Royal Charter in 1673 as part of Christ's Hospital to train forty King's Scholars in the art of navigation.

The superb production of these absorbing volumes reflects the greatest credit on all concerned, including the famous Press that has printed them, and particularly on Professor Turnbull whose notes are a most valuable aid to our understanding of the points at issue. Indeed, he draws our attention to many that might otherwise be missed. Both volumes have been fully indexed, but it would have been an added advantage if the tables of contents had contained under each item a brief reference to the main topics discussed in it, instead of merely stating the writer, the addressee, and the date.

G. J. WHITROW

Reason and Chance in Scientific Discovery. By R. Taton.
Hutchinson, London, 1957. Pp. 171. 30s.

MOST British and American writers on the History and Methodology of Science have merely described when the discoveries in their sciences were made or have concentrated upon the logic of discovery. They have been interested in the formal connection between the discoveries they have chronicled, either temporally, or philosophically as within one world system or another. In either case they have regarded the act of discovery itself as outside their province. The logic of deduction and induction is discussed in philosophical terms, and lip service is paid to the psychological aspects of the processes involved.

Dr Taton refreshingly introduces his readers to the psychological problems involved in the act of discovery. His work has been well translated by A. J. Pomerans. He describes, with copious examples, some of the factors involved in discovery—flashes of thought, the rôle of chance and the rôle of error. He also describes what is involved in an original discovery and how some discoveries have been missed by great scientists working so close to the discovery that future generations have been forced to wonder why it was missed. The examples chosen are from many different fields and form a useful starting point for thought. One is struck by the number of continental references to writers also interested in this aspect of the philosophy of science. It would be of considerable importance to have other works in this field made more easily available for English speaking readers.

How successful is Taton's account? I think its success and its defect lies in its superficiality. Reading his account one can arrive at certain morals for the would-be scientist. One pictures the perseverance of the Curies, and can see that perseverance is, morally, a good thing in a scientist. Or one can see that without a broad theoretical grasp of a scientific situation one is going to be unable to see the relevance of a particular observation. There are many such moral precepts embedded in this book. However, this does not add up to the psychology of genius or the psychology of invention. We need to know much more about the psychology of the people described before we can be convinced of the real steps by which they associatedpreviously unassociated phenomena. We need to know more about the psychology of explanation and the motivations for it in a particular scientist's case before we can more than scratch at the surface of scientific theorising. One gets the impression that Dr Taton believes that the unconscious behaves as though its logic is that of the conscious. We have known since Freud that this is not so, and it is precisely in this field of unconscious associations that flashes of thought have their psychological coherence. Nowhere in the book is there a discussion of the obsessionalism that makes one worker wash his mistakes down the sink and that makes another notice a bactericidal fungus. Fleming is discussed, but not as a person.

Dr Taton is to be congratulated in making a step in the direction of the psychology of discovery. However, one wonders whether his goal can ever be achieved by writing history, for it is impossible to build up a science of the psychology of discovery by reference to untestable historical data. Much more needs to be done in the present before we can assess the correctness of his historical psychological theorising.

R. F. J. WITHERS

Variational Principles in Dynamics and Quantum Theory. By Wolfgang Yourgrau and Stanley Mandelstam.
Sir Isaac Pitman and Sons, London, 1960. Pp. xi + 180. 32s. 6d.

IF someone intended to follow the history of physical theory as a search for harmony, continuity, economy, conservation, and 'reason' in general, he could hardly find a more lively exposition than Yourgrau and Mandelstam's *Variational Principles* whose second edition of 1960 is augmented by a timely chapter on quantum field theory. Ample quotations from the sources, far more revealing than the usual short notes quoted from one book to another, are combined with an extensive presentation of the mathematical material. Beginning with the discovery of 'nature's parsimony' by Hero and Fermat in optics, and by Maupertuis in mechanics, we learn of the increasing recognition during the eighteenth century that minimum theorems are not to be taken as signs of reasonableness in nature's plan but as mathematically convenient transformations of mechanical laws, established before in differential form: If one has a differential equation, one can always find some quantity which is conserved, and some other quantity which under certain restrictions becomes an extremum, without *a priori* telling which quantity ought to have the prominent quality in question. Following the authors in their presentation of natural philosophy from Pythagoras to Aristotle, then *via* Ockham, Bacon, and Kepler to the mechanics of Newton, and to the integral principles of Maupertuis, Euler, Lagrange, Hamilton, and Jacobi, one is impressed by the increasing elegance and generality of the variational method as a powerful tool for the solution of intricate classical (as well as quantum) problems, testifying to 'the continuous transformation of a metaphysical canon to an exact natural law'.

Of particular interest is the mechanisation of the Maxwell-Lorentz field equations, which by their division into oscillators become adaptable to the Hamiltonian method and thereby also to quantisation. The untraditional mathematical method of Feynman, which rests on an integral condition rather than on a differential wave equation, is presented in readable form, to be recommended to any student of this difficult subject. Although it is not a variational method, Feynman's approach generates one under semi-classical conditions—analogous to Fermat's minimum principle which holds for optics under similar restrictions. It is a particular merit of the authors to have shown that Schwinger's method, though independently arrived at, is derivable from, hence equivalent to, Feynman's integral principle. These intricate mathematical developments are reduced by the authors to a minimum of formal complexity and a maximum of lucidity. The broad historical perspective of the book written in brilliant style, not forgetting the human angle, turns a tedious chapter of mathematical physics into a delightful reading matter also for the not-so-mathematical student interested in the development of mechanics from its stumbling beginnings to modern quantum mechanics of fields.

ALFRED LANDÉ

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1961

26 October: 'Minds and Machines'
Norbert Wiener, Mathematics, M.I.T.
S. Watanabe, I.B.M. Research Center, Yorktown Heights, N.Y.
30 November: 'Language and the Formation of Concepts'
E. H. Lenneberg, Harvard University
Commentator: Roger Brown, M.I.T.
14 December: 'On the Conceptual Structure of Physics'
Laszo Tisza, Physics, M.I.T.

1962

8 February: 'Modal Logics I: Foundations'
Ruth Barcan Marcus, Philosophy, Roosevelt University
Commentator: W. V. Quine, Philosophy, Harvard University
1 March: 'Modal Logics II: The Formal Analysis of Cultural Objects'
Alan Ross Anderson, Philosophy, Yale University
O. K. Moore, Sociology, Yale University
22 March: 'Interpretations of Quantum Theory'
Ralph Schiller, Theoretical Physics, Stevens Institute of Technology
Armand Siegel, Theoretical Physics, Stevens Institute of Technology
Commentator: Abner Shimony, Theoretical Physics, Boston University and M.I.T.
26 April: 'The Falsifiability of Theories: Total or Partial? A Contemporary Evaluation of Duhem's Hypothesis'
Adoph Grünbaum, Philosophy of Science, Pittsburgh University
Commentator: R. S. Cohen, Physics, Boston University
10 May: 'Perception: Language and Perceptual Schemes'
Noam Chomsky, Linguistics, M.I.T.
Sidney Morgenbesser, Philosophy, Columbia University

For further information on the Colloquium, write to Professor M. W. Wartofsky, c/o Department of Philosophy, Boston University, Boston 15, Massachusetts.

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CLASSICAL AND NON-CLASSICAL CONCEPTS IN THE QUANTUM THEORY *

An Answer to Heisenberg's *Physics and Philosophy*

DAVID BOHM

Physics and Philosophy† by W. Heisenberg makes a very useful contribution to the literature on the interpretation of the quantum theory. It is of interest, first of all, because it can be regarded as an authoritative presentation of what has come to be known as the 'Copenhagen' point of view (because of its association with Niels Bohr). Professor Heisenberg has played a leading role, both in the development of the quantum theory itself, and in the formulation of its interpretation according to the Copenhagen school. He is therefore eminently qualified to discuss this subject.

In view of the scarcity of clear expressions of the Copenhagen interpretation, this book is to be welcomed, particularly because in it many essential points which have hitherto been only implicit consequences of remarks in obscure articles are brought out openly and explicitly. By the same token, the author's efforts to relate his view to the natural science, the history of physics, and to philosophy in general, are illuminating, while his answers to criticisms of the Copenhagen interpretation will certainly be of interest to all those who are concerned with the subject, either from a scientific or a philosophical point of view.

Because this book presents the basic features of the Copenhagen interpretation in such a clear light, it constitutes a useful basis on which further criticisms of this interpretation can be developed. An article of this kind does not provide adequate space for an exhaustive analysis along these lines, but it may be of interest here to go into a few of the most important points involved.

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* Received 4.1.61. This article was originally planned as a review of *Physics and Philosophy* by Werner Heisenberg. But since Heisenberg and the author of this article actually have criticised each other before, and since this debate is partly continued by Heisenberg in *Physics and Philosophy*, it may be fairer to describe this article as an answer to Heisenberg rather than a review.

† W. Heisenberg, *Physics and Philosophy*, London, 1959.

First of all, with regard to the uncertainty principle, Heisenberg[1] brings out a point which has not been sufficiently emphasised elsewhere; namely, that the ambiguities which arise in the measurement process are only a special case of a universal ambiguity in the very mode of existence of everything. What is important here is not the impossibility of our obtaining perfectly precise knowledge about micro-systems, which follows in a well-known way from the disturbance due to the quanta by which the apparatus must interact with the observed system. Rather, the essential point is, as Heisenberg states, that 'a large part of the universe, including ourselves, does *not* belong to the object'. (By 'object', Heisenberg means here the particular object under investigation.)

It therefore appears to be recognised in this book that the underlying basis of the uncertainty principle is the real interconnection of everything in the universe, by means of quanta. Because these quanta are *indivisible*, they belong as much to one side of the connection as to the other. An essential aspect of what each thing *is* therefore lies in all other things, with which it is thus indivisibly related. From this, it follows that precisely definable modes of description of the detailed attributes of a thing (e.g. in terms of continuous mathematical variables) must be subject to a minimum degree of ambiguity, *this ambiguity being objective and universal, and not restricted just to the relationship between objects and observing apparatus.*

It follows from the above described point of view that the common notion of what is called an 'object' must be changed in a fundamental way. For nothing (neither an electron nor an observing apparatus nor a man nor anything else) can correctly be regarded, even abstractly and conceptually, as having at each moment a complete and separate existence, the state of which is merely modified with the passage of time by its interaction with other things. Rather, as we have already indicated, there is an interpenetration in the very modes of existence of different things, such that only in the classical limit is the approximation of separately existing objects valid. This interpenetration can, in principle, include even a human being who observes the world and in the same act participates in it. It is with this possibility in mind that

[1] op. cit., bottom of p. 52

Heisenberg[1] calls attention to the need to give up the attitude which he calls ' objectivisation ', that is, the use of the concept of the separately existing object beyond its proper domain of validity. On the other hand, we are not thereby led to fall into the opposite error, termed by him ' subjectivism ', that is, the tendency to regard all our experience (e.g. measurements and observations) as referring only to our private knowledge and not to a real world of which we are a part. For in all cases of experimentation in physics (as well as in practical applications), the major source of interconnection with the observed system is by way of the apparatus, while the human being has direct contact only with the latter, which being in the large-scale domain, is influenced in a negligible way by the contact. It follows then that in physics, the universe as a whole, *including the apparatus*, can for practical purposes be regarded as a separate object. However, no distinction between the object of investigation and the observing apparatus can be made at a quantum level of accuracy, nor can a perfectly sharp distinction be made between any two objects whatsoever in the universe, whether they are part of an experiment or not.

In the further development of his exposition of this notion, however, Heisenberg introduces ideas which do not seem to be entirely consistent with the above described point of view. Thus he states[2] that in the quantum theory the probability function (derived in the well known way from the wave function) combines objective and subjective elements. It contains statements about possibilities or potentialities, which are ' completely objective, not depending on any observer.' However, he further asserts, the probability, ' contains statements about our knowledge of the system, which are subjective insofar as they may be different for different observers '. Moreover, he later goes on to say ' the observation itself changes the probability function discontinuously; it selects of all possible events the actual one that has taken place.' Therefore, he concludes, ' the transition from the " possible " to the " actual " takes place during the act of observation '. Thus Heisenberg seems to be saying here that whereas possibilities can exist outside the human mind, *actuality* can only be when someone perceives it.

In the next step, however, he recognises that this view is evidently not adequate, and in order to deal with this problem in another way, he introduces a distinction between the ' psychic ' and the ' physical ' act of observation.[3] He defines the physical act of observation as the process of interaction of the measuring apparatus with the observed system; and in this process, he further states:

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[1] op. cit., p. 75 [2] op. cit., p. 53 [3] op. cit., p. 53

> The transition from the 'possible' to the 'actual' takes place as soon as the interaction of the object with the measuring device, and thereby with the rest of the world, has come into play; it is not connected with the act of registration of the result by the mind of the observer. The discontinuous change in the probability function, however, takes place with the act of registration, because it is the discontinuous change of our knowledge in the instant of registration that has its image in the discontinuous change of probability function.

Heisenberg has thus brought into consideration two kinds of actualities,[1] namely, 'the psychic' and the 'physical'. In order to bring out in more detail the kind of problem to which this leads, we shall consider specific examples of measurement of the position of an electron, with the aid of a set of Geiger counters arranged on a grid so as to cover the region of interest. Let us suppose then that the electron has already entered the system and triggered off one of the counters, but that no observer has yet looked to see *which* counter this is. Now, as Heisenberg indicated, at this stage one knows what are the objective physical *possibilities*; namely, that the counter in question must be one of those located where the amplitude of the electron wave function (and therefore the probability function) is appreciable. If, however, one tries to describe the fact that there is also what Heisenberg called 'physical actuality' (viz. that among the above described possibilities, some counter has actually functioned), one discovers that there is no way in the theory to do so. For, as Heisenberg points out, the wave function, with its associated probabilities (which must, of course, contain all the information about the observed system that there can be) describes only 'psychic' actualities. In other words, until an observer *actually perceives which counter has operated*, so that he can write a new wave function representing the actual state to which the previous possibilities have 'collapsed' as a result of his perception, there is, as far as anything that can appear in the theory is concerned, no actuality at all, but only the above described set of possibilities.[2] The physical actualities introduced by Heisenberg therefore play no part whatsoever in the theory, since no predicted result would be changed in any way at all if the theory were developed without mentioning them. Nevertheless, Heisenberg evidently feels that it would not be permissible to leave them out of consideration altogether, for then we would be left only with 'psychic' actualities, and therefore, with a subjectivist point of view, which he quite justifiably wishes to avoid. It would appear then that in order to keep away from subjectivism, he is ready to adopt the completely 'metaphysical' assumption of physical actualities which play no part at all in the theory, but which are introduced only to avoid what he would otherwise regard as an untenable philosophical position.

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[1] The word 'actuality' refers, as its root implies, to some kind of *act*. In Heisenberg's discussion of the problem, this act is an observation.

[2] This conclusion is not altered if we treat both the apparatus and the observed system quantum mechanically. For, in this case, there will be a wave function of the combined system, which after interaction is over, but before an observer has perceived the result, will still spread over the same range of possibilities as was obtained when we treated only the observed system quantum-mechanically. For a more detailed discussion, see, for example, D. Bohm, *Quantum Theory*, New York, 1951, chap. 22.

If one wishes to deal with this problem in a more adequate way, it would seem almost inevitable that one will have somehow to introduce further physical conceptions and associated mathematical functions, which could represent the state of the 'physical' actuality as existing outside the mind of the observer who perceives it. These new 'actuality functions' would supplement the wave function, which latter would then constitute only one side of the theory. Naturally, the theory would have to be developed in such a way as to define the relationship between the actuality functions and the wave function, as well as to bring out the relationship of both to experiment. Of course, this could be done adequately only by in some way enriching, developing, and extending the laws of the quantum theory, so that the physical actualities would cease to be purely 'metaphysical', and would instead play a real and essential role, both in the theory and in experiment.

It was on the basis of a consideration of the problem described above that the writer of this article was first led to criticise the Copenhagen interpretation. While trying to find a way to remedy the absence of 'actuality functions' he developed a definite example of an alternative interpretation, which permitted the quantum theory to be extended so as to include them in a logically consistent way.[1] In this interpretation, it was proposed, in connection with an electron, for example, that in addition to the Schrödinger wave function, there was also a particle with a well-defined position and momentum, which interacted with the wave in a certain prescribed manner. The position of this particle plays the part here of an actuality function, in the sense that when the wave function spreads out over many possibilities, this particle determines which of these possibilities is actually present.

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[1] D. Bohm, *Phys. Rev.*, 1952, 85, 166; 180. See also *Causality and Chance in Modern Physics*, London, 1957.

In his book, Heisenberg devotes considerable attention to the above criticism of the Copenhagen point of view as well as to many other criticisms that have been made by various authors. With regard to his discussion of the proposals described above (which alone will be considered here), Heisenberg's answer divides naturally into two parts.

In the first part,[1] he begins by objecting to various aspects of the model described above, stating that they are physically implausible and mathematically inelegant,[2] without, however, criticising either the logical consistency of the model, or its ability to account for all the phenomena that are treated in the Copenhagen interpretation. In doing this, he shows that he perhaps did not appreciate that the only purpose of this phase of the work was to show that an alternative to the Copenhagen interpretation is at least *logically possible*. If one such interpretation is possible, then it is very likely that others will be possible too. Therefore, it should be reasonable to go on, using the original model as a starting point, with the aim of developing a formulation that is more plausible physically and elegant mathematically. (Toward the end of this article, some results of the writer's further efforts in this direction will be indicated.)

At this point, however, Heisenberg raises another kind of objection, which leads him to question whether the above described programme is feasible at all. For, as he points out, the suggested new interpretation leads to precisely the same predictions for all experimental results as are given by the Copenhagen interpretation. Therefore, he argues that the introduction of the new particle variables, playing the role of actuality functions, is only a kind of 'ideological superstructure', which can never lead to anything physically new.

The possibility of such a criticism was, however, already taken into account by the writer of this review in the original articles referred to above (as well as in subsequent work on the subject). Indeed, with this problem in view, it was noted in these articles that the new interpretation constituted a broadening of the conceptual framework, permitting the introduction of a wide range of possible modifications, that could not even be expressed in the Copenhagen interpretation.

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[1] op. cit., p. 114

[2] In particular, he objects especially strongly to its failure to maintain certain kinds of symmetry in the formalism, such as that between position and momentum.

Several illustrative examples of such modifications were actually outlined, but they were not described in detail, because the writer did not regard this particular model as a good basis for a definitive theory. Nevertheless, they were carried far enough to show that the suggested actuality functions (i.e. the particle variables) were capable of playing a part that could, in principle, lead to theoretical predictions that were quite different from those that are possible in the Copenhagen interpretation. It was also suggested that such differences would be important mainly in new domains not at present very well understood or investigated (e.g. that of short distances), while it was clear from the way the theory was formulated that the usual results could be obtained as a good approximation in the domain where the quantum theory has thus far been carefully studied.

It is in the second part of his discussion of this topic, however, that Heisenberg makes his main point with regard to the possibility of developing alternatives to the Copenhagen interpretation. For, as can be seen from his further comments [1] on the above described suggestions for modifying the quantum theory, he does not believe that this can be done in any way at all. He gives his reason in a quotation from Niels Bohr, saying that the expression of such an aim is similar to the sentence: 'We may hope that it will later turn out that sometimes $2 + 2 = 5$.' In other words, the proponents of the Copenhagen interpretation regard the development of any alternative to their point of view as logically impossible.

It must be emphasised, however, that the above contention is not proved, neither in Heisenberg's book nor elsewhere in the literature. Therefore, if we wish to criticise it, the best that we can do is first to explain it by reconstructing the probable lines along which such a proof might be attempted. In order to do this, we shall begin with the stress laid by Heisenberg [2] on the essential character for the Copenhagen interpretation of 'the use of classical concepts in describing our experimental equipment and, more generally, in describing that part of the world which does not belong to the object'. In the section following this quotation, it is made clear that by the term 'classical concepts', one understands, firstly, space and time, and secondly, energy and momentum. In classical physics, the concepts of space and time refer to a kinematic description of the process of movement of matter; for example, the series of positions occupied by an object in a sequence of times. The momentum and energy concepts refer, however, to a description of the causal laws that hold in this movement; for example, Newton's laws, that the time rate of change of the momentum of a body is equal to the force applied to it (which leads in an isolated system to the conservation of momentum and energy). These laws are, of course, completely deterministic, in the sense that the initial positions and momenta of all the bodies of an isolated system completely determine the future behaviour of that system.

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[1] op. cit., pp. 117 and 118

[2] op. cit., p. 82

As is well known, in the quantum theory, the laws are not completely deterministic, but instead take the very different form of determining statistical relations. What is more important for our purpose here, however, is that the concepts (space, time, energy and momentum) in terms of which one expresses the experimental implications of quantum mechanical laws are nevertheless the same ones used in classical mechanics. In other words, as Bohr frequently emphasises,[1] the quantum theory introduces no new concepts into physics. What is new in this connection is, however, that *the old concepts, such as position and momentum, can be defined only within maximum limits of precision set by the uncertainty principle.* This ambiguity applies even to large-scale objects, but, of course, its consequences with respect to these are unimportant for practical purposes. Nevertheless, from the point of view of principle, the above described ambiguity plays a very significant part. For it means that from the experimental data entering into physics (and into any other science) one will be able to infer the properties of various parts of the universe to a maximum degree of precision which is exactly the same as that implied by the current quantum theory, when these properties are treated in the usual way in terms of wave functions, operators, etc. As a result there will be no room for the possibility of inferring anything of a different character (such as our proposed particle variables representing 'actuality functions'), which would aim to add some new features to the description of the state of the system under observation, features that are not expressible in terms of the formalism of the quantum theory as it now stands. This would mean, however, that alternatives to the Copenhagen point of view could never have any additional experimental content, and so would be, as Heisenberg suggested, just 'metaphysical' additions to the description, fulfilling no further function.

In view of the far reaching consequences of the conclusion described above, it is evidently important to ask what evidence there is in favour of its basic premise; namely, that the results of large scale experience must be expressed in classical concepts. In discussing this question, Heisenberg[1] states:

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[1] Private communication

> The concepts of classical physics are just a refinement of the concepts of daily life and are an essential part of the language which forms the basis of *all natural science*.[2] Our actual situation is that we *do* use the classical concepts for the description of experiments. . . . There is no use discussing what could be done if we were other beings than we are.

It is clear from the above quotation that Heisenberg feels that classical concepts are inherent in all logical thinking about any subject whatsoever. Such a point of view implies that every understandable and describable aspect of experience could in principle be analysed by regarding the world as made up of various component parts, each having at any moment a definite position and a definite momentum. If we in practice do not do this in everyday life, but use other concepts instead, this means only that we are approximating to the ideal of such a complete analysis (i.e. by 'refining' these everyday concepts, we could come to nothing else but a suitable set of positions and momenta).

It must be admitted that ideas of the type described above are commonly held by physicists. It takes only a little reflection, however, to see that there are strong reasons suggesting that such ideas are false. To see why this is so, let us concentrate on an essential feature of the concepts of space and time as they are used in classical physics, namely, the description of the location of an object or an event in terms of a set of *continuous Cartesian co-ordinates.* Is it really true that we have no other way to think of space and time except in terms of such a concept? We may ask, for example, whether in everyday experience we would describe the location of a pencil by giving its precise latitude and longitude (which is what is implied by the above described 'classical' concept of space). It is evident that we do not do this, but that instead, we describe the pencil as being on a certain desk, which is in a certain room, which is in a certain house, on a certain street, etc. In other words, we locate the pencil with the aid of a series of *topological relations,* in which one entity is *within* or *upon* another. The same is true of all laboratory experience. For in no experiment does one ever give an exact co-ordinate of anything (i.e. to an infinite number of decimals). Rather, in a typical measurement, one places a point *between* certain marks on a scale, thus once again locating it by means of a topological relationship. Indeed, in every experiment that can possibly be done, the notion of a precisely defined co-ordinate is seen to be just an abstraction, which is carried out when a topologically described experimental result is translated into the language of continuous co-ordinates.

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[1] op. cit., p. 55

[2] Italics added

It is clear then that behind the concept of space and time as described in terms of co-ordinates, there is a more fundamental topological concept, which lies at its basis, but which is usually just taken for granted without further discussion. For evidently, the idea of defining co-ordinates makes sense only if one presupposes that one knows what is meant by topological relations such as between, inside, outside, neighbourhood, etc. This is true not only with regard to the actual physical procedures by which measurements are carried out, but also with regard to the order of logical priority in the definition of the associated mathematical concepts. Thus, if the mathematical theory of space and time is to be developed in a natural way, it is well known that one must begin with topological relations, such as those described above,[1] and that only much later will one arrive at the concept of Cartesian co-ordinates (which is essentially a metrical notion).

We see then that Heisenberg's contention that classical concepts are the only possible refinements of everyday concepts must be false. For as shown above, one can at the very least obtain refinements leading to topological concepts, which are at once closer to everyday experience and more fundamental in a logical mathematical sense. (Of course, refinements of everyday concepts in still other directions should be possible too; but for physics, topology is particularly important, since it deals with the description of space and time, which play a central role in all physical theory.) We have therefore refuted the basic premise underlying the thesis of the inevitability of the Copenhagen interpretation, namely, that the only possible concepts are those of classical physics.

At this point, however, we are led naturally to go further, and to raise the question of just how topology can play an essential role in the quantum theory.[2] To do this, we shall give here a brief sketch of some of the lines along which the writer of this review has been developing a *topological formulation of the quantum theory*,[3] which constitutes a counter-example to the Copenhagen point of view. In this way, we shall be able to indicate how ' non-classical ' concepts can play a basic part in physical theory, making possible new kinds of experimental predictions which could not be considered within the framework of the Copenhagen interpretation, and which, according to Heisenberg's conclusions, should not be possible at all.

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[1] See, for example, A. Wallace, *Algebraic Topology*, London, 1957

[2] The idea that there may be a fundamental relationship between quantum theory and topology was first suggested, to the author's knowledge, by L. L. Whyte. See, for example, his *Critique of Physics*, London, 1931.

[3] Some of the mathematical aspects of this formulation are still being developed, but it is expected that the work will be ready for publication in the near future.

Our basic starting point in this formulation is that space and time are to be described by topological concepts (which are taken to be axiomatic) while concepts involving co-ordinates will play only a secondary role. This means that we do not begin, as is done in some topological theories,[1] from continuous co-ordinates to which differential equations are assumed to apply ' in the small ' and then deduce topological relations ' in the large '. Rather, the fundamental equations with which we start must be expressed, from the outset, in terms of topological notions applying to *space and time* (such as before, after, between, inside, outside, overlap, boundary, path connectivity, etc.).

The relation between continuous Cartesian co-ordinates and discrete topological concepts of the type described above can be brought out in more detail by means of the following highly simplified illustration. Consider an arbitrary point, P, on a string of unit length. We first fold the string in two. If P is on the right side of the fold, we assign it a ' topological co-ordinate ', $C_1 = 1$; if on the left, $C_1 = 0$. We continue to fold it some finite number, N, of times. In this way, we will obtain N topological co-ordinates for the point. These co-ordinates will give us a topological description of where the point is, which locates it as being within certain regions, which in turn are inside others, outside still others, etc. (Rather as with the case of the location of a pencil in common experience, as described previously.) In the limit as N approaches infinity, the set of the c's can be used to define a continuous Cartesian co-ordinate, $x = \sum_{0}^{\alpha} \frac{c_n}{2^n}$, (which expresses x as a binary decimal). In the ' classical ' point of view, we regard the limit of the above sum as the essential item of information, which must enter into the formulation of physical laws (e.g. in a differential equation), while the process of summation of the infinite series is regarded as irrelevant for this purpose. On the other hand, in the topological point of view, we regard the coefficients of the series as containing the essential information which must enter into physical laws, while the sum of the series is considered to be only an abstraction (applicable in limiting cases when the number of elements is very large).

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[1] Such an approach to physics has recently been made by several authors; for example, J. A. Wheeler, *Phys. Rev.*, 1955, **97**, 511

It is clear then that the *laws* of physics will no longer be expressed as differential equations in a continuous set of Cartesian co-ordinates, but instead as topological relations in a discrete set of topological co-ordinates. Some clue as to the precise nature of these topological relations can be obtained from the existence of a remarkable analogy between the mathematics of topology and that of the modern quantum mechanical field theory (which is generally recognised to be the most definitive expression of the laws of the quantum theory available to date). Even with the simplified example of a topological co-ordinate given above, one can see the existence of some kind of analogy. For the fundamental field variables of the quantum theory contain ' number of particle (Fermion) operators ' associated with each point in space and time, which resemble our above described topological co-ordinates in having possible values that are either zero or unity. Thus, what is suggested is that perhaps the deeper meaning of these quantum mechanical operators is that they describe topological relations in space and time. This suggestion is in fact supported by a more detailed study (which we cannot enter into here), showing that certain groups which are basic in topology are also basic in the quantum mechanical field theory. From the group isomorphism, it is possible to show that our topological theory leads naturally, in the limit in which the approximation of continuous co-ordinates can be applied, to a Fermion field satisfying Dirac's equation (and of course, the Pauli exclusion principle).[1]

It appears then that the utilisation of the above described analogy can make possible the development of a topological formulation of the quantum theory, which while leading to the results of the usual quantum mechanics in suitable limiting cases, nevertheless possesses certain genuinely novel features, with regard both to its mathematical formulation and to its experimental predictions. In order to bring out in more detail what these novel features are, we first point out that in the topological formulation, a fundamental concept will be that of an *event*. In contradistinction to the ' classical ' axiom that fundamental events are to be described in the theory as mathematical points, we shall start with the axiom that all events (even those used in the expression of the basic laws of physics) are to be considered as constituting regions, having some spatial extension and temporal duration.

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[1] Boson fields are also shown to have a certain topological significance, which we shall not, however, go into here.

('Objects' will then consist of repetitive, persistent, and organised patterns of events.) A fundamental relationship into which such events can enter is that of *containment* of one event inside another. (For example, a flash of light can be in the space between a certain pair of marks on a ruler and occur while a given clock pointer is between a certain pair of marks on its dial.) This relationship replaces the classical one of coincidence, which evidently can be applied meaningfully only to point events. Of course, for the development of a physical theory it is not sufficient to specify the containment of one event in another. A whole system of containment relations is needed, which would in effect constitute an extension of the kind of topological co-ordinates that we have described above, so as to set up a fairly detailed topological description of what is commonly called the 'space-time frame'.

The next important concept is that of *elementary process.* An elementary process is a relationship between two different events, in which one becomes the other. Such an elementary process has by its very definition an aspect of *indivisibility.* For even though it is *potentially divisible,* if such a division were actually to be carried out, it would become a *different process.* (As for example, an egg can be divided by breaking it, but it thereby ceases to be an egg.)[1] In this way, we see that the basic quantum mechanical feature of the indivisibility of elementary processes fits naturally into topological concepts of space and time, whereas it does violence to the 'classical' conceptions, which are inherently those of continuity.

From the elementary process, the next step is to consider the total cosmic process, in which all events that have been, are, and will be, are in principle taken into account, in their proper order (described topologically of course). Naturally, we cannot in practice specify all the details of this process; but as is done regularly in the science of cosmology, we can consider some model of the cosmos, which aims to treat only certain idealised and simplified systems of events, that are relevant in the discussion of some limited problem of interest (e.g. an experiment). In this connection, the observing apparatus, being a part of the cosmos that is evidently, in general, significant for the experiment, must also be described in the theory, at least in a suitably idealised and simplified way. (As, for example, Heisenberg did in his well known model of a highly idealised microscope with the aid of which he illustrated the uncertainty principle.) However, as we saw earlier, it is agreed, even by the proponents of the Copenhagen point of view, that at least in physics, the human observer can be left out of the description, since his act of perceiving the state of the large scale apparatus makes no significant change in the latter.

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[1] A similar result is obtained even in the Copenhagen interpretation. For example, by observing a quantum process, we can divide it as a result of the disturbance due to the measuring apparatus, but then it ceases to be the same process that was originally under discussion.

The system of events described above is then the ' physical actuality ' that, we saw previously, Heisenberg felt to be necessary, even though in the Copenhagen point of view, it plays no physical role. In the topological formulation, however, the situation is different. For here one *begins* with the actual process with all its events in their proper order. To each event in the process, one then associates a particular wave function, characteristic of that event. In effect, every such event is regarded as furnishing a unique and individual perspective on the cosmic process (in the sense that in a certain way it ' perceives ' its past, in the form of a trace or a set of marks left by this past in this particular event). As a detailed development of the theory shows, the perspective of a given event is represented mathematically by the wave function associated with that event. This wave function then determines potentialities for the development of the cosmos subsequent to the event in question.

It is clear from the above discussion that in our formulation of the quantum theory, there is an inexhaustible set of wave functions (one for each event), whereas in the usual formulation, there is, as is well known, only one wave function (or at most, one for each observer). To relate the way the wave function appears in both points of view, we note that in our theory, the (large scale) events which are the results of the functioning of a particular piece of apparatus furnish their own special perspective on the cosmic process. When an observer looks at the apparatus (of course, without changing it significantly) and perceives these events, he can, if he wishes, adopt as his own the perspective on the cosmic process that is implied by the events in question, and with it the corresponding special associated wave function. This wave function implies potentialities for the future development of the system under discussion, in the same way as happens in the usual formulation of the theory, and therefore plays a role analogous to that of the wave function as it appears in the Copenhagen interpretation. It is clear, however, that our point of view differs from that of the Copenhagen interpretation in the essential respect that in it, the wave function does not primarily represent ' psychic ' actuality (i.e. the perception of the event by an observer), but rather, 'physical' actuality (i.e. the perspective of the associated event on the cosmic process).

The introduction of one wave function for each event must lead, of course, to new features, both in the mathematical formulation of the theory, and in the mode of relating it to experiment. Thus, with regard to the formulation, it is evidently necessary to ensure that the potentialities implied by the wave function of any one event are compatible with those implied by the wave functions of all the other events, so that they will fit together to make a coherent whole. The steps needed to satisfy this requirement lead in fact to a further mathematical development, into the details of which we shall not enter here. In essence, however, what is done is to determine the future of a given set of events as the logical intersection of all the potentialities implied by this set of events (i.e. what is common to all these potentialities). A little reflection will show that this idea is quite natural, being only a refinement of what is done in 'common sense' with regard to similar problems. For we are merely requiring that actuality be that which is possible, according to every valid perspective. The determination (or limitation of range of possibilities) brought about in this way need not be complete, but as more perspectives are taken into account, it can in general go beyond the limits set by the uncertainty principle, which latter follow when we suppose that only a single perspective is available.

The type of determination described above is quite different from that appearing either in classical theory or in quantum theory. For in the latter two theories, the basic procedure is to start with some initial conditions (e.g. precise positions and momenta in classical theory, or a definite 'quantum state' in quantum mechanics), and then to predict the corresponding entities to be found in a future experiment. As a result, one simply discards a great deal of information contained in the *observable sequence of events* leading up to the initial conditions; for it is effectively assumed that nothing matters except the initial conditions themselves, independently of the process in which they came to be what they are. On the other hand, there is no logical justification for discarding the additional information which is in the order of this sequence (beyond of course, the practical one that in a certain limited range of phenomena in which current theories are satisfactory, this order is found not to be relevant). And indeed, in the topological formulation of the quantum theory, the additional information described above is taken into account. For in this formulation, the sequence of actual events not only appears directly in the fundamental mathematical laws (which are expressed in terms of the wave functions associated with each event in the sequence), but it also appears as part of the basic empirical data of any problem (because the observed sequence helps to determine, or at least to limit, the wave functions described above).

We conclude then that, contrary to what is maintained in the Copenhagen interpretation, the quantum theory can be developed further to introduce the notion of a ' physical actuality ', with a new experimental and mathematical content not expressible in the usual formulation, and nevertheless approaching the current theory in those domains in which the latter has really been well confirmed. In order to achieve this result, however, we have been led to introduce topological concepts that are quite different from those appearing in classical physics and in the Copenhagen interpretation of the quantum theory. The applicability of such topological concepts is not restricted to the micro-domain, but rather, extends through every level, including the large scale level of common experience. For example, the movement of a particle is no longer to be described in terms of the classical concepts of position and momentum, but rather in terms of a series of discrete events.[1] The events are so close together in space and time, however, that for a certain limited range of practical purposes, they may be treated on the large scale level as forming a continuous orbit. (As the pressure on a macroscopic object, due to a rain of molecules on its surface, can similarly be treated as continuous for some purposes, even though we must keep the actual atomic constitution of matter in mind when we are trying to understand pressure in a fundamental way.)

It follows then that the use of topological concepts implies the need for a re-evaluation of the significance of *every aspect of physics* including not only the current quantum theory but also classical mechanics. When this re-evaluation is accomplished, however, it is discovered that we are simply giving up the excessively abstract classical notion of Cartesian co-ordinates, and returning to a topological notion of space and time that is very close to one that we have always been using in common experience.

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[1] The change between one event and its successor takes the place of the concept of momentum.

CAUSALITY IN CONTEMPORARY PHYSICS (II) *

M. Schlick

Translated by David Rynin

9 *Is the Principle of Causality in the Quantum Theory False or Empty?*

In order to understand the situation it is necessary to compare two formulations that the criticism of the causal principle assumes in physics. Some say that the quantum theory has shown (of course presupposing that it is correct in its present form) that the principle is *not valid* in nature. The others say that it is *empty*. The former believe thus that it makes a definite assertion about reality that experience has proven false; the others believe that the proposition in which it is apparently expressed is not at all a genuine assertion but is a meaningless succession of words.

As evidence for the first point of view, Heisenberg's oft cited article (in *Z. Physik*, 1927, 43) is quoted, which says: 'Because all experiments are subject to the laws of quantum mechanics, the invalidity of the causal law is definitively determined by quantum mechanics.' Born is commonly named as representing the second point of view (cf. the passage cited above). Hugo Bergmann ('Der Kampf um das Kausalgesetz in der jüngsten Physik', Braunschweig, 1929) and Thilo Vogel ('Zur Erkenntnistheorie der quantentheoretischen Grundbegriffe' Diss., Giessen, 1929) have concerned themselves with the philosophical aspects of this dilemma. Both of these authors correctly assume that those physicists who reject the causal principle are none the less of the same opinion basically, even if they say different things, and that the apparent difference is to be attributed to the inexact language of the one party. Both are of the opinion that it is Heisenberg who is guilty of the inexactness, and that therefore it should not be said that the quantum theory has proven the principle to be *false*. Both emphasise the fact that the causal law can neither be affirmed nor rejected by experience. Shall we consider this interpretation as correct?

First we must affirm that we consider the bases upon which H. Bergmann forms his opinion to be quite incorrect. For him the causal law is to be neither discredited nor affirmed, because he considers it to be a synthetic judgment *a priori* in the *Kantian* sense. On the one hand, as is well known, such a judgment is supposed to express a genuine cognition (this is conveyed by the word ' synthetic '), on the other hand it must be incapable of test by experience because ' the possibility of experience ' rests upon it (that is conveyed by the words *a priori*). We know today that these two requirements contradict each other; there are no synthetic judgments *a priori*. If a proposition says anything at all about reality (and only when it does so does it contain knowledge) its truth or falsity must be determinable by observation of reality. If there is no possibility of such a test *in principle*, if the proposition is compatible with every experience, it must be empty and cannot contain any knowledge of nature. If, on the assumption of the falsity of the proposition, something in the world of experience were different from what it would be if the proposition were true, then of course it could be tested. Consequently not-testable-through-experience means that the way the world appears to us is quite independent of the truth or falsity of the proposition, hence it says nothing about the world. Kant, of course, believed that the principle of causality says a gr[redacted]e empirical world, even determines its essential natur at deal about th does no favour to Kantianism or *a-priorism* when [redacted]. Therefore on principle cannot be tested. With this we have [redacted]ne affirms that tha's point of view (the same would hold true o[redacted]rejected H. Bergman[redacted] so far as he inclines towards a moderated,[redacted] Th. Vogel's opinion [redacted] formulations at the end of the treatise ci[redacted] *a priorism*; however hi[redacted]) and we must therefore consider a [redacted]cted are not quite clear to me[redacted] falsity of the principle of [redacted]w phase of the question. Does [redacted] causality really follow from results of quantum mechanics? [redacted] it follow rather that the proposition is without content?

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* Continued from the previous number

A sequence of words may be meaningless in two ways: either it is tautological (empty) or it is not a proposition at all, not an assertion in the logical sense. It would appear at first sight as if the latter possibility did not enter into our considerations here, for if the words that are to express the causal principle do not represent a real proposition, they must simply be a meaningless, absurd succession of words. One must, however, bear in mind that there are sequences of words that are not propositions and express no facts and yet fulfil very significant functions in life; so-called *question* and *command* sentences. And even if the causal principle is expressed in the grammatical form of a declarative sentence, we know from modern logic that one can hardly judge the logical content of a sentence by its form. And thus it is quite possible that beneath the categorical form of the causal principle a kind of command, a demand exists—thus, approximately, what Kant calls a 'regulative principle'. A similar opinion regarding this principle is indeed held by those philosophers who see in it merely the expression of a postulate or of a 'decision'[1] never to give up the quest for laws and causes. This point of view therefore must be carefully considered.

Accordingly, we must decide between the following three possibilities:

I. The principle of causality is a tautology. In this case it would always be true but without content.

II. It is an empirical proposition. In this case it could be either true or false, either knowledge or error.

III. It represents a postulate, an injunction to continue to seek causes. In this case it cannot be either true or false but is at most either appropriate or inappropriate.

I. We shall soon become clear regarding the first possibility, especially as we have already mentioned it above (§ 6). We found that the causal principle as expressed in the form, 'All events occur according to law', is certainly tautological if by lawfulness is meant, 'representable by some formula or other'. From this, however, we inferred that this could not be the true content of the principle, and we looked for a new formulation. In fact, science in principle has no interest in a tautological proposition. If the causal principle were of this nature, determinism would be self-evident, but empty. And indeterminism, its opposite, would be self-contradictory, for the negation of a tautology is a contradiction. The question as to which one of the two is correct could not be raised at all. Therefore if modern physics not only formulates the question but believes it to be definitely answered by experience, what physics means by determinism and the principle of causality surely cannot be a tautology. In order to know whether a proposition is tautological or not one obviously needs no experience at all, one need only realise its meaning. If one should say that physics has demonstrated the tautological nature of the causal principle, it would be as senseless as to say that astronomy has shown that 2 times 2 equals 4.

Since the time of Poincaré, we have learned to note that apparently certain general statements enter into a description of nature which are not subject to confirmation or disproval, namely the 'conventions'.

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[1] H. Gomperz, *Das Problem der Willensfreiheit*, 1907

The genuine conventions, which are actually a type of definition, must in fact be formulated as tautologies. Here, however, it is not necessary to go further into this matter. We conclude only that since we have already admitted that modern physics at any rate teaches us something about the validity of the principle of causality, it cannot be an empty proposition, a tautology, a convention; but it must be of such a nature that in some way it is subject to the judgment of experience.

II. Is the principle of causality simply a proposition the truth or falsity of which may be determined by observation of nature? Our previous considerations seem to support this interpretation. If it is correct we shall have to side with Heisenberg, therefore, in opposition to H. Bergmann and Th. Vogel in the above mentioned apparent opposition between the formulations of Heisenberg and Born, in which these investigators express the results of the quantum theory. I call that opposition apparent, for while Heisenberg speaks of the invalidity and Born of the senselessness of the causal principle, Born yet adds ' in its usual formulation '. Therefore it may well be that the usual formulation gives rise to nothing but a tautology, but that the real meaning of the principle could be formulated in a genuine statement which could be proven false by quantum experiments. In order to determine this we must again consider which formulation of the causal principle we found ourselves driven to accept. According to our former statements the content of the principle may be expressed thus: ' All events are in principle predictable.' If this statement were a genuine proposition it would be verifiable—and not only this, but we would be able to say that the verification has been attempted and has so far given a negative result.

But what is the case with our principle? Can the meaning of the word ' predictable ' be clearly indicated? We called an event ' predicted ' when it was deduced by the help of a formula that was constructed on the basis of a series of observations of other events. Mathematically expressed, prediction is an extrapolation. The denial of exact predictability, as the quantum theory teaches, would mean then that it is impossible to derive from a series of observations a formula that will also represent, exactly, *new* observation data. But what again does this ' impossible ' mean? One may, as we saw, *subsequently* always find a function that includes the new as well as the old data. A rule may, therefore, always be found that connects the previous data with the new data and makes both appear derivative from the same natural regularity. That impossibility is therefore not a *logical* one;

it does not mean that *there is no* formula with the desired properties. Strictly speaking, however, it is also not a real impossibility; for it is possible that someone should by mere chance, by pure guess, always get the correct formula. No natural law prevents correct guesses regarding the future. No, that impossibility means that it is impossible to *seek* that formula, that is, there is no *rule* for obtaining such a formula. This, however, cannot be expressed in a legitimate proposition.

Our efforts to find a testable proposition equivalent to the causal principle have therefore failed. Our attempts at formulations have led only to pseudo-propositions. This result is not entirely unexpected, however, for we have already said that the causal principle may be tested for its correctness *in the same sense* that a natural law may be tested. We also noted, however, that natural laws, strictly analysed, are not propositions that are true or false, but are, rather, ' directions ' for the construction of such propositions. If this holds also of the causal principle, we find ourselves referred to the third possibility:

III. The principle of causality does not directly express a fact to us, say, about the regularity of the world, but it constitutes an imperative, a precept to seek regularity, to describe events by laws. Such a direction is not true or false but is good or bad, useful or useless. And what quantum physics teaches us is just this: that the principle is *bad*, useless, impracticable within the limits precisely laid down by the principle of indeterminacy. Within those limits it is impossible to seek for causes. Quantum mechanics actually teaches us this, and thus gives us a guiding thread to the activity that is called investigation of nature, an opposing rule against the causal principle.

Here one sees again how much the situation created by physics is different from the possibilities that have been thought out by philosophy. The causal principle is no *postulate* in the sense in which this concept occurs in earlier philosophers, for there it means a rule to which we must adhere *under all circumstances.* Experience, however, decides upon the causal principle, of course, not upon its truth or falsity—that would be senseless—but upon its utility. And natural laws themselves decide the limits of utility. In this lies the novelty of the situation. There are no postulates in the sense of the older philosophy. Each postulate may be limited by an opposing rule taken from experience, that is, may be recognised as inappropriate and thus nullified.

One might perhaps believe that this point of view would lead to a type of pragmatism, since the validity of natural laws and of causality depends only on their *confirmation* and on nothing else. But here there is a big difference that must be sharply emphasised. The statement of pragmatism that the truth of propositions consists entirely in their confirmation, in their usefulness, must from our standpoint be rejected. Truth and confirmation are not identical for us. On the contrary, since in the case of the causal principle we may test only its confirmation, only the usefulness of its precept, we may not speak of its ' truth ', and we deny to it the nature of a genuine statement. Of course, pragmatism may be understood psychologically and its teaching may, as it were, be excused, by saying that it is really difficult and requires thorough reflection to see the difference between a true proposition and useful rule, between a false proposition and useless rule. For ' directions ' of this type occur grammatically in the form of ordinary propositions.

While for a real assertion it is essential that it be in principle verifiable or falsifiable, the usefulness of a direction can never be absolutely proven because later observations may still prove it to be inappropriate. The Heisenberg relation itself expresses a natural law, and as such has the nature of a direction. On this basis alone the rejection of determinism arising therefrom cannot be considered proof of the falsity of a definite proposition, but may be considered only an indication of the inadequacy of a rule. The hope therefore always remains that with further knowledge the causal principle will again triumph.

The expert will observe that by considerations such as the above the so-called problem of ' induction ', too, ceases to have application, and is thus solved in the way in which Hume solved it. For the problem of induction consists in the question of the logical justification of genera propositions regarding reality, which are always extrapolations from individual observations. We recognise, as Hume does, that there is no logical justification for them; there cannot be one because they are not real propositions. Natural laws are not (in the logician's language) ' general implications ', because they cannot be verified for *all* cases, but are rules, instructions, aiding the investigator to find his way about in reality, to discover true propositions, to expect certain events. This expectation, this practical attitude, is what Hume expresses by the word ' belief'. We must not forget that observation and experimentation are *actions* by which we come into direct contact with nature. The relations between reality and us are sometimes expressed in sentences that have the grammatical form of propositions, but whose real meaning lies in their being directives for possible action.

To sum up: The rejection of determinism by modern physics means neither the falsity nor the emptiness of a definite proposition of the ψ waves, and in the occurrence of a particle or a quantum, which is simply accidental, within the limits of ' probability ' determined by the ψ value at the point concerned. (That is, the value of ψ tells us, for instance, that at a definite point *on the average* 1,000 quanta per second enter. These 1,000, however, show quite an irregular distribution in themselves.)

What does ' simply accidental ' or ' chance ' or ' completely unordered ' mean here? From the previous case, of the regular occurrence of A and B together in an average of 99 per cent of the observations, which does not represent complete order, we may by gradual transitions pass to disorder. Let us assume, say, that observation shows that on the average the process B follows the process A in 50 per cent of the cases, the process C follows A in 40 per cent, and D follows A in the remaining 10 per cent. We should still speak of a definite regularity, of statistical causality, but we should then judge a much smaller degree of order to be present than in the first case. (A metaphysician would perhaps say that the process A has a certain ' tendency ' to call forth the process B, a slighter tendency toward the process C, etc.) When would we state that there is *no kind* of regularity, that therefore the events A, B, C, D are completely independent of one another (in which case the metaphysician would say that there is no inherent tendency in A to produce its consequent)?

Obviously only when, after a very long sequence of observations every series formed out of the different events by permutation (with repetition) occurs on the average with the same frequency (where the series would have to be small with respect to the whole sequence of observations). We should then say that nature has no predilection for a definite succession of processes, that the succession therefore takes place quite irregularly. Such a distribution of the events has usually been called a distribution ' according to the rules of probability '. Where such a distribution exists we speak of complete independence of the events in question; we say they are not causally connected with one another. And according to what has been said, this manner of speech does not signify merely an *indication* of the lack of regularity but is *identical* with it by definition. The so-called probability distribution is simply the *definition* of complete disorder, pure chance. It seems to be generally admitted that to speak of ' laws of chance ' is a very poor way of expressing the matter (since chance means the exact opposite of law). One too easily tends to ask the meaningless question (the so-called ' problem of application ' relates to this) how it happens that even chance is subject to law. I cannot therefore accept Reichenbach's point of view when he speaks of a 'principle of distribution according to the law of probability' as a presupposition of all the natural sciences; which principle, along with the principle of causality, is to form the basis of all physical knowledge. That principle, he thinks, consists in the assumption that the irrelevant factors in a causal relationship, the 'remaining factors', 'exert their influence in accordance with the law of probability'.[1] It seems to me that these 'laws of probability' are nothing more than the *definition* of causal independence.

Of course we must here include an observation that although without practical significance is yet of great importance, logically as well as in principle. The above definition of absolute disorder (equifrequent average occurrence of all possible sequences of events) would be correct only in the case of an infinite number of observations. For it must be valid for series of any magnitude and every one of these must, according to the previous observation, be regardable as small in comparison with the total number of cases, that is, the total number of cases must surpass all limits. Since in reality this, of course, is impossible, we cannot, strictly speaking, decide *whether* disorder conclusively exists in any case. That this must be so follows, moreover, from our previous result, that for an already given sequence we cannot decide whether it is 'orderly' or not. The same difficulty of principle exists here that makes it impossible to define the probability of any event in nature by the relative frequency of its occurrence. To arrive at correct estimates such as are required for mathematical computation (probability *calculations*) we should have to pass to the limit for an infinite number of cases—naturally a senseless demand for empiricism. This is often not sufficiently considered.[2] The only useful method of defining probabilities is the one of *Spielraüme*, logical range (Bolzano, V. Kries, Wittgenstein, Waismann; see the above cited article of Waismann).

This, however, does not belong to our theme. We now proceed to derive some consequences from the above considerations and to criticise others that are drawn here and there in this connection.

11 *What does 'determined' mean?*

Since, generally, we speak of causality by saying that one process *determines* another, that the future is *determined* by the present, we want once more to clarify for ourselves the true significance of this unhappy word ' determine '. That a certain state determines another later one can in the first place *not* mean that there is a hidden connection between them called causality, which could somehow be found or must be thought. For such naïve ways of thinking are, for us, surely no longer possible, 200 years after Hume. Now we have already given the positive answer, at the beginning of our deliberations: ' A determines B ' cannot mean anything but: B may be *calculated* from A. And this again means: there is a general formula which describes the state B as soon as certain values of the ' initial state ' A are put into it, and as soon as a certain value is given to certain variables, for instance that of time, *t*. That the formula is ' general ' means, again, that besides A and B there are any number of other states connected with one another by the same formula and in the same manner. Indeed a large part of our efforts was directed to answering the question of when one may say there *is* such a formula (called ' natural law '). And the answer was that the criterion lay in nothing but the actual observation of the B computed from A. Only when we can indicate a formula that is used successfully in prediction can we say that there *is* a formula (order is present).

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[1] 'Kausalstruktur der Welt', p. 134

[2] Cf. for instance R. von Mises, *Wahrscheinlichkeit, Statistik und Wahrheit*, 1928; English translation: *Probability, Statistics and Truth*, 1939.

The word ' determined ' therefore means exactly the same as ' predictable ' or ' computable in advance '. This simple viewpoint alone is needed to resolve a well-known paradox, important for the problem of causality, which perplexed Aristotle and is even today a source of confusion. It is the paradox of so-called ' logical determinism '. It states that the principles of contradiction and of excluded middle would not be valid for propositions regarding future facts if determinism is not valid. In fact, so Aristotle argued, if indeterminism is correct, if the future is not already *determined*, it seems that the proposition, ' the event E will take place the day after tomorrow ', can be today neither true nor false. For if, for instance, it were true the event *would have to* occur, it would already be determined, contrary to the indeterministic assumption. Even in our day this argument is sometimes held to be conclusive, and is even said to be the basis of a new logic.[1] There must of course be an error here, for logical propositions, which are only rules of our symbolism, cannot depend as regards their validity upon the existence of causality in the world; every proposition must have truth or falsity as a timeless characteristic. The correct interpretation of determinism removes the difficulty at once and leaves to the logical principles their validity. The proposition 'the event E occurs on such and such a day' is timeless—therefore even now either true or false, and only one of the two, quite independently of whether determinism or indeterminism holds in the world. For the latter by no means asserts that today the proposition about the future E is not unambiguously true or false, but only that the truth or falsity of that proposition cannot be *calculated* on the basis of propositions about present events. This means, then, that we cannot *know* whether the proposition is true before the corresponding point in time has passed, but this has nothing to do with its being true or with the basic laws of logic.

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[1] Cf. J. Lukasiewicz, 'Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls ', *Comptes rendus des séances de la Société des Sciences et des Lettres de Varsovie*, 1930, pp. 63 ff.

12 *Determination of the Past*

If physics today, speaking indeterministically, says that the future is (within certain limits) *undetermined*, it means nothing more or less than: it is impossible to find a formula by which we may calculate the future from the present. (More correctly it would mean: it is impossible to *seek* such a formula, there is no rule for its discovery; it could be *guessed* only by pure chance.) It is perhaps comforting to observe that in quite the same sense (and I cannot imagine any other meaning of the word 'undetermined') we must say of the past that in a certain respect it, too, is undetermined. Let us assume for example that the velocity of an electron has been precisely measured and then its location observed: in this case the equations of the quantum theory also enable us to compute, *exactly*, previous positions of the electron.[1] However, actually, this indication of position is physically meaningless, for its correctness cannot in principle be tested, since it is impossible to verify—subsequently—whether the electron appeared in the computed place at the given time. *If*, however, *one had* observed it in this computed place, it would certainly not have reached those places later noted, since its course is known to be disturbed by observation in an incalculable manner. Heisenberg says (p. 15): 'Whether or not one attributes a physical reality to the calculation of the past of the electron is merely a matter of taste.' However, I should prefer to express myself even more strongly, in complete agreement with what I believe to be the fundamental point of view of Bohr and Heisenberg himself. If an

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[1] Heisenberg also emphasises this: *Die Physikalischen Prinzipien der Quantentheorie*, 1930, p. 15.

assertion regarding the position of an electron is not verifiable in atomic dimensions, then we cannot attribute any meaning to it; it becomes impossible to speak of the 'path' of a particle between two points where it has been observed. (This of course is not true of bodies of molar dimensions. If a bullet is now here and a second later at a distance of 10 metres, then it must have passed the points in between during that second, *even if no one has perceived it*; for in principle it is possible to verify subsequently that it has been at the intervening points.) One may treat this as the sharpened formulation of a proposition of the general theory of relativity: just as transformations that leave all point coincidences—intersection points of world lines—unchanged have no physical meaning, so we may say here, there is no sense at all in attributing physical reality to the segments of world-lines between the points of intersection.

The most concise description of the state of affairs we have discussed is perhaps to say (as the most important investigators of the quantum problems do) that the validity of the usual spatio-temporal concepts is limited to that which is macroscopically observable; they are not applicable to atomic dimensions.

None the less let us spend another moment with the results just arrived at concerning determination of the past. We sometimes find it asserted in current literature that contemporary physics has re-established the ancient Aristotelian concept of 'final cause', in the form of that which is earlier being determined by the later, but not vice versa. This idea occurs in the interpretation of the formulas of atomic radiation which, according to the theory of Bohr, is supposed to take place so that the atom sends out a light quantum every time an electron jumps from a higher to a lower orbit. The frequency of the light quantum depends upon the initial orbit *and the final orbit* of the electron (it is proportional to the difference in energy values of the two orbits); it is therefore obviously determined by a *future* event (the entrance of the electron into the final orbit).

Let us test the meaning of this idea. Aside from the fact that the concept of final cause must have had a different content for Aristotle, this idea, according to our analysis of 'determine', states that in certain cases it is impossible to compute a future event Z from the data of past events V, but that, on the other hand, V may be derived from the known Z. Good, let us imagine that the formula for this is given and that a V has been computed therefrom. How do we test the correctness of the formula? Only by comparing that which is computed with the observed V. V, however, is already in the past (it existed before Z, which has also already occurred and had to be known in order to be insertable into the formula); it cannot be observed *post festum*. If then we have not previously ascertained it, the proposition that the computed V occurred is not verifiable in principle and is therefore meaningless. If however V has already been observed, we have a formula that connects events already observed. There is no reason why such a formula should not be reversible. (For in practice *one-many* functions do not occur in physics.) If by means of it V may be calculated from Z, it must be equally possible to determine Z by means of it when V is given. We therefore encounter a contradiction when we say the past may be calculated from the present but not vice versa. Logically both are the same. Note well: the essence of this argument is that the data of the events V and Z enter into the natural law with entirely equal right; they must all already have been observed if the formula is to be verifiable.

For the rest, here too, all the obscurities are basically due to the lack of clear distinction between that which may be formulated as a contribution of thought and that which has really been observed. Here again we see the great advantage of Heisenberg's point of view, which would offer a purely mathematical and not an apparently intuitive model of the atom; with it the temptation to introduce so-called final causes falls to the ground. It seems to me that the mere elucidation of the meaning of the word 'determine' shows that it is under all circumstances impermissible to assume (quite independently of the question of determinism) that a later event determines an earlier one, but that the reverse is not true.

13 *Toward Differentiation of Past and Future*

The last considerations seem to teach that an inference regarding past events has precisely the same nature logically as one regarding future events. In so far as, and to the extent that, causality holds at all, we may say with equal justice that the earlier determines the later and the later determines the earlier. In accordance with this all attempts to differentiate conceptually between the temporal direction from the past to the future and from future to past fail. This I believe is true also of H. Reichenbach's attempt (in the treatise cited in the *Bayrischen Sitzungsberichten*) to demonstrate the asymmetry of the causal relationship, and by its help to ascertain conceptually the positive temporal direction and thereby to be able to define even the time of the present, the now. He believes that the causal structure in the direction of the future differs topologically from that of the reverse direction. The arguments he gives for this belief I consider incorrect. However, I do not want to dwell upon this (compare for instance the critique, in need of some further elaboration, of Reichenbach's ideas by H. Bergmann in 'Der Kampf um das Kausalgesetz in der jüngsten Physik') but merely to mention that the demand for a definition of the now is logically meaningless. The difference between earlier and later in physics may be described objectively, and in fact, as far as I can see, only by aid of the principle of entropy. But in this way the direction past-future is only *differentiated* from the opposite. However, that real events *proceed* in the first direction and not in the reverse cannot be said at all, and no natural law can express it. Eddington (*The Nature of the Physical World*) describes this in an intuitive way, in claiming that a positive temporal direction (time's arrow) may be defined physically, but that it is not possible to formulate conceptually the passage from the past to the future (becoming). H. Bergmann rightly sees, in opposition to H. Reichenbach, that physics has no means whatever of distinguishing the now, of defining the concept of the present. He seems, however, to assume falsely that by means of 'psychological categories' this may not be impossible. In truth the meaning of the word now may only be *shown*, just as we may only show and not define what we understand by 'blue' or by 'happiness'.

That the causal relation is asymmetrical, unidirectional (as Reichenbach, loc. cit. believes) is falsely suggested by facts connected with the principle of entropy. It is only due to this law that in everyday life the earlier may be more readily derived from the later than vice versa. The calculation of the later is of course not by itself identical with an inference to the future, and neither is the calculation of earlier itself identical with an inference to the past. This is the case only when the temporal point from which we make the inference is the present. Reichenbach believes (loc. cit., p. 155) that the latter case is actually distinguished by the fact that the past is objectively determined whereas the future is objectively undetermined. Brief analyses show that all that is meant by 'objectively determined' is 'inferable from a partial effect'. The future is 'objectively undetermined' because it cannot be inferred from a partial cause, for the totality of all partial causes can not be defined in the absence of determinism. All sorts of things may be said against the concepts of partial cause and partial effect, and we have already indicated that the apparently easier process of inference is falsely suggested by facts involved in the principle of entropy. But even if the argument contained no error it would again only characterise the difference between the earlier and later, not the difference between past and future.

14 *Indeterminateness of Nature and Freedom of the Will*

The psychological reason for the sort of ideas last mentioned (and that is why I referred to them) seems to me to lie in the circumstance that, in addition to the simple meaning our analysis found for the word 'undetermined', implicitly a sort of metaphysical, related meaning is attributed to it; namely, as if one could attribute determinateness or indeterminateness to a process *in itself.* That, however, is meaningless. Since 'determined' means calculable by means of certain data, to speak of determinism makes sense only when we add: *By what?* Each real process whether it belongs to the past or to the future is as it is; being undetermined cannot belong to its characteristics. Regarding the natural processes themselves one cannot sensibly assert a 'vagueness' or 'indefiniteness'. Only in reference to our thoughts may we speak of such (namely, when we do not know definitely which propositions are true, which representations are correct.) Sommerfeld evidently means just this when he says[1]: 'It is not the experimentally ascertained things that are indeterminate. With sufficient attention to experimental conditions, these may be precisely treated. Indeterminism applies only to our ideational forms which accompany physical facts.' One must not believe therefore that modern physics has any place for the misconception of natural processes 'undetermined in themselves'. If, for instance, in an experiment it is not possible to give an electron a precise location, and if the same is true of its momentum, this means nothing more than that position and momentum values of a punctiform electron are not suitable means for the description of the process that takes place in nature. The modern formulations of the quantum theory recognise this and take it into account.

Just as little as the present situation in modern physics allows the formulation of a metaphysical concept of indeterminism does it allow speculations about the so-called problem of freedom of the will which is connected with it. This must be sharply emphasised, for not only philosophers but also men of science have not been able to withstand the temptation to utter thoughts such as the following: Science shows us that the physical universe is not fully determined; it follows, (1) that indeterminism is in the right and that physics therefore does not contradict the assertion of freedom of the will; (2) that nature, since strict causality does not prevail in it, provides room for spiritual or mental factors.

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[1] *Scientia* [Milano], 1930, 8, 85

In answer to (1) we may say: the real problem regarding freedom of the will as it occurs in ethics has been confused with the question of indeterminism only because of crude errors which, since Hume, have long since been corrected. The moral freedom that the concept of responsibility presupposes does not stand in opposition to causality but would be entirely destroyed without it.[1]

To (2) we may say: the statement implies a dualism, the juxtaposition of a spiritual and physical world between which there may be an interaction because of the imperfect causality of the latter. In my opinion no philosopher has succeeded in elucidating the real *meaning* of such a proposition, that is, no one has shown which experiences would enable us to confirm its truth and which experiences would disclose its falsity. Quite the contrary, logical analysis (for which of course there is no place here) leads to the conclusion that in the data of experience there is no legitimate ground for that dualism. It is therefore a meaningless, untestable metaphysical proposition. It seems to be believed that the possibility of ' psychical ' factors entering through possible loopholes of ' physical ' causality has consequences relating to our world outlook that satisfy certain emotional needs. However this is an illusion (since the purely theoretical interpretation of the world has no relationship to emotional needs correctly understood). If the tiny gaps in causality could in some way be filled in, it would only mean that the above mentioned, practically insignificant traces of indeterminism existent in the modern world-picture would again be partly wiped out.

In this realm the metaphysics of earlier times was guilty of certain errors which sometimes occur even now where metaphysical motives are completely absent. Thus we read in Reichenbach (p. 141): ' If determinism is correct nothing can justify our undertaking an action for tomorrow but not for yesterday. It is clear that we then have no *possibility* at all even to abstain from the *plan* for the morrow's action and from the *belief* in freedom—certainly not, but in that case our action does not make *sense*.' It seems to me that the exact opposite is the case: our actions and plans obviously make sense *only* in so far as the future is determined by them. Here there is simply a confusion of determinism with fatalism, which has so often been criticised in the literature that we need not dwell upon it. Moreover, he who still represents the opinion criticised above would not be helped at all by the indeterminism of modern physics. For with utmost consideration of all relevant facts, happenings are still so precisely calculable beforehand in it, the remaining indeterminateness is so slight, that the significance our actions would have in this world of ours would still be vanishingly small.

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[1] Cf. my *Fragen der Ethik*, Chapter 7, 1930; English translation: *Problems of Ethics*, New York, 1939

Precisely the last considerations teach us again how different the contributions of modern physics to the question of causality are from those of earlier philosophical thinking; and how correct we were in saying at the very beginning that human imagination was in no position to foresee the structure of the world as revealed to us by patient investigation. For it is even difficult for it to progress in the steps science has already shown to be possible.

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(*Concluded*)

DISCOVERING THE POSITRON (II) *

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3 *The Universal Reluctance to Accept a New Particle*

How is one to explain the uniform resistance everywhere shown to the idea of a ' third ' particle? Or, as Millikan puts it:

> Prior to the night of August 2, 1932, the fundamental building stones of the physical world had been universally supposed to be simply protons and negative-electrons. Out of these two primordial entities all of the 92 elements had been formed.[1]

How is one to explain the fact that Rutherford, the experimentalist, and Bohr, the theoretician, felt that, whatever Anderson actually observed, its interpretation as a new particle must be in error?

How is one to explain Dirac's energetic attempts to construe the ' negative-energy ' solutions as representing protons? His ultimate concession of a new particle was diffident and reluctant. Schrödinger too, did everything to avoid this ' third particle ' conclusion. And recall Oppenheimer's wish to ' . . . return to the assumption of two independent elementary particles, of opposite charge and dissimilar mass . . . '.

And how is one to explain the pre-Anderson photographs of tracks now readily identifiable as having been made by positive electrons? Most of these remained unnoticed, or were characterised as ' spurious ', or ' dirt effects '; or they were left uninterpreted, or were wrongly interpreted, as with some tracks seen by Skobeltzyn, Blackett, and the Joliot-Curies.

There must be some general explanation of this reaction against there being a positive electron. Explore with me, on a slight detour from our main route, the conceptual background which was designed to exclude a third particle. Perceiving this will throw the discoveries of Anderson, Dirac, and Blackett into high relief. It will also explain the quickening of research and experimentation within particle theory during the 1930s.

Our answers lie within the development of the theory of electricity.

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* Part I appeared in the previous number

[1] Millikan, op. cit. p. 320

'Electron' was the ancient Greek word for amber. The Greeks knew that rubbed amber attracts other objects to it. Gilbert (1600) found that glass, when rubbed, had the same property. The glass was 'electrified', that is, amberised. Dufay (1698-1739) discovered that sealing wax, rubbed with cat's fur, was also electrified, but differently from the glass. The wax attracted all electrified bodies which had been repelled by the glass, and repelled those attracted by the glass. Dufay remarks that here are '. . . two electricities of a totally different nature . . ., transparent solids . . ., or resinous bodies: . . . Each of them repels bodies which have contracted an electricity of the same nature as its own, and attracts those whose electricity is of the contrary nature.' Dufay called these two types of electricity 'resinous' and 'vitreous'—names still used. In 1747 Benjamin Franklin also recognised these two forms of electricity. He arbitrarily called them 'positive' and 'negative'. A body was then negatively charged if repelled by sealing wax rubbed with cat's fur. It was positively charged if repelled by a glass rod rubbed with silk. These definitions still obtain.

Franklin also discovered the conservation of electric charge. He noted that electricity is not created by rubbing the glass, but only transferred to the glass from the material rubbed against it. The latter loses what the glass gains. Hence the total quantity of electricity in any insulated system is invariable. Franklin considers the material rubbed against the glass as deficient in electricity; he says it has a charge of $-e$. But the glass, however, now has a superfluity of electricity, and a charge of $+e$. So Dufay's 'vitreous electricity' is identical with Franklin's electrical fluid. And 'resinous electricity' is the absence of that electrical fluid, or rather, a deficiency of that 'fluid' possessed by all ponderable bodies. Thus Franklin's electrical fluid exists as a natural constituent of all matter in the neutral, unelectrified state. A superfluity of this naturally-appearing electrical fluid gives the body a positive charge. A deficiency results in negative charge.

Here, then, is a clear conception of electrification, of positive and negative charge.

The possibility of electricity also being particulate, or granular, or atomic, in character gets serious attention in the nineteenth century. In 1851 Joule determined the average speed of molecules moving in a given gas at ordinary temperature.[1] In 1860 Clerk Maxwell determined the mean distance a molecule moves between collisions.[2]

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[1] $v = 1$ mile/sec.: Cf. *Mem. of the Manchester Lit. and Phil. Soc.* 1951, 2nd ser., p. 107; *Phil. Mag.*, 1857, p. 211

[2] Maxwell, *Phil. Mag.*, **19**, 28

Clausius had already discussed this, but Maxwell gave the first evaluation, utilisable within the general theory of the viscosity of gases. He also estimated the number of molecules in a cc. of gas. Ostwald resisted every form of the atomic hypothesis until the last edition of his *Outlines of Chemistry*. The isolation and counting of gas ions, the Brownian motion, and the kinetic theory ultimately forced him to accept the particulate position.

Even Franklin had written 'the electrical matter consists of particles extremely subtle, since it permeates common matter, even the densest, with such freedom and ease as not to receive any appreciable resistance.' In 1833 Faraday found that a given amount of electricity passing through a solution containing H, or a compound of H, caused the same amount of hydrogen to appear at the negative terminal, irrespective of the compound or the strength of the solution. Moreover, the quantity of electricity required to make one gm. of H appear always deposited from a silver-solution exactly 107·05 grams of Ag. Since the Ag atom is 107·05 times heavier than the H atom, and both are associated in the solution with exactly the same amount of electricity, it was concluded that all univalent atoms, i.e. those which combine with one atom of H, carry the same amount of electricity. All bivalent atoms carry twice this amount. Valency, then, is proportional to the amount of electricity carried by the atom in question.

However, all the scientific reflection up to this time took electricity to exist *on* a charged body. It was assumed to exert forces on other charged bodies just as does gravity. To Faraday this was nothing but action at a distance; it was totally repugnant to him. He learned that the electrical force between two charged bodies depends in some way on the nature of the intervening medium. Gravitational pulls are independent of the medium. Consequently, electrical forces are transmitted rather as the elastic deformation at one end of a rod is transmitted along the rod.

Clerk Maxwell puts the point thus: '. . . it is in questionable scientific taste, after using atoms so freely to get rid of forces acting at sensible distances, to make the whole function of the atoms an action at insensible distances . . .'.[1]

However, unlike transmission of elastic deformations, electrical forces act through a vacuum. This led directly to the infamous aether, whose history it is not our purpose to discuss.

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[1] Cf. Maxwell, 'Atom' in *Scientific Papers*, ii, p. 480

These concepts were put into a powerful mathematical form by James Clerk Maxwell. Attention was now drawn from phenomena in or on a conductor and refocused on the ' stresses and strains ' in the medium surrounding the conductor. When Hertz, in 1887, found that electrical forces were transmitted as waves, at precisely the speed of light predicted by the Faraday-Maxwell theory, the triumph of the aether-stress view seemed complete. Professor Oliver Lodge, indeed, says that electrical charge is only ' a state of strain in the aether ', and a current is nothing other than a ' continuous breakdown of a strain in the medium within the wire '.[1] But this ' strain theory ' led men to think of the strain distributed evenly over the surface of a charged body, rather than as it was observed to do, namely, radiate from centres all over the surface of the body.

Concerning the particulate character of electricity, then, between 1833 and 1900 the physicists

(1) thought of the passage of electricity through a solution as a motion of specks or atoms, each carrying an exact multiple, between one and eight, of some elementary electrical atom, and

(2) thought of the passage of a current through a metal as a continuous ' slip ' or ' breakdown ' of a strain in the matter itself.

(1) and (2) were felt to be different in kind; electrolytic conduction and metallic conduction. The latter was much more prominent. Hence the atomic conception was only slightly heeded. Maxwell notes the problem. He says, ' for convenience in description we may call this constant molecular charge (revealed by Faraday's experiments) one molecule of electricity'.[2] But he then goes on to deny any real physical significance to this convenient decision, saying

> it is extremely improbable that when we come to understand the true nature of electrolysis we shall retain in any form the theory of molecular charges, for then we shall have obtained a secure basis on which to form a true theory of electric *currents* and so become independent of these provisional hypotheses.

And after all, Maxwell was justified in granting that an ion always took into the solution a definite quantity of electricity, without thereby conceding that the charge on the electrode is made up of the same number of electrical atoms.

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[1] Lodge, *Modern Views of Electricity*

[2] Maxwell, *Electricity and Magneticism*, ' Electrolysis ', 1873.

At about the same time, Weber[1] built up an entire theory on the modified Franklin-idea. Weber explained all electrical phenomena of conduction, including thermodynamic and Peltier effects, by assuming two types of electrical constituents of atoms. One of these was taken to be more mobile than the other. Thus Ampère's molecular current, which rendered molecules into tiny electro-magnets, Weber conceives as the rotation of light positive charges about heavy negative ones.

He writes:

> The relation of the two particles, as regards their motions, is determined by the ratio of their masses e and e′, on the assumption that in e and e′ are included the masses of the ponderable atoms which are attached to the electrical atoms. Let e be the positive electrical particle. Let the negative be exactly equal and opposite, and therefore denoted by — e (instead of e′). But let a ponderable atom be attracted to the latter so that its mass is thereby so greatly increased as to make the mass of the positive particle vanishingly small in comparison. The particle — e may then be thought of as at rest, and the particle + e as in motion about the particle — e. The two unlike particles in the condition described constitute then an Amperian molecular current.[2]

Except for the fact that it is the *negative* particle whose mass is negligible compared with the positive, and not *vice versa* as Weber conjectures, the quotation above contains precisely the idea which Lorentz later developed into modern electron theory.

In 1874 Stoney stated the atomic theory of electricity. He even estimated the value of the elementary electrical charge [$\cdot 3 \times 10^{-10}$ absolute electrostatic units].[3] Stoney writes:

> . . . nature presents us with a single definite quantity of electricity which is independent of the particular bodies acted on. . . . I shall express Faraday's law in the following terms . . .: *for each chemical bond which is ruptured within an electrolyte a second quantity of electricity traverses the electrolyte which is the same in all cases.* This definite quantity of electricity I shall call e_1. If we make this our unit of electricity, we shall probably have made a very important step in our study of molecular phenomena . . .[4]

And compare Helmholtz writing in 1881:

> . . . if we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also,

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[1] That is, in 1871, Weber, *Werke*, iv, 281

[2] Weber, loc. cit.

[3] Stoney, ' On the Physical Units of Nature ', *Phil. Mag.*, 1881, **12**, 384

[4] Loc. cit.

> positive as well as negative, is divided into definite elementary portions which behave like atoms of electricity.[1]

Even Kelvin, otherwise disposed to regard electricity as a 'continuous homogeneous liquid' writes :

> Faraday's laws of electrolysis seem to necessitate something atomic in electricity . . .[2]

The term 'electron' was coined in 1891 by G. Johnstone Stoney. This was his designation for the 'natural unit of electricity', that is that quantity of electricity which must pass through a solution in order to liberate at an electrode one atom of hydrogen or of any univalent substance.

> A charge of this amount is associated in the chemical atom with each bond. There may, accordingly, be several such charges in one chemical atom, and there appear to be at least two in each atom. These charges which it will be convenient to call 'electrons' cannot be removed from the atom, but they become disguised when atoms chemically unite.[3]

Consider the semantical moral behind this short history. For Stoney 'electron' denoted the elementary quantity of electricity; this and nothing more. Hence Stoney feels that every atom contains at least two such electrons, one positive and one negative. Otherwise ordinary matter would not remain electrically neutral. But 'electron' was rapidly appropriated for other uses as well. The dynamical particle we now call the 'free negative electron', has a mass of 1/1835 of the H atom. This massy particle also 'has' the fundamental unit of electricity ; $e = 4{\cdot}80 \times 10^{10}$ esu. That is, the particulate electron also *has* the electron as one of its properties ! Only this latter property was intended by Stoney as the electron. The 'physical object' connotations so prominent in the work of J. J. Thomson were just becoming apparent at the turn of the century.

However, Stoney's use of 'electron' continued in use. Thus Thomson, Rutherford, Campbell, and Richardson, particularly after 1913, always talk of positive as well as of negative electrons. The mass associated with the former, that is, the positive electron, was usually that of the H-atom itself. But these physicists are always referring to the positive elementary charge (found on the nucleus of the H-atom), and the negative elementary charge (found on the particle called 'electron'). Nernst clearly defined the positive and negative electrons as the elementary positive and elementary negative electrical charges respectively.[1]

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[1] Helmholtz, *Wissenschaftliche Abhandlungen*, 3, 69

[2] Kelvin, 'Contact Electricity and Electrolysis', *Nature*, 1897, 56, 84

[3] Stoney, *Scientific Transactions of the Royal Dublin Society*, 1891, 11 series, 4, 563

So, during the first World War, 'electron' could be construed either as the fundamental unit of electrical charge (positive or negative), or as the dynamical material particle one of whose properties was to carry that unit of charge. Positive electrons and negative electrons were freely discussed. But by 1919, when the proton's existence had been definitely established, the terminology within elementary particle physics had become intricate. The 'positive electron' was now the proton. And the 'negative electron' was now the electron.[2]

Beyond this, however, such an intimate association between the two elementary particles, and the two basic units of electricity—this made the very idea of a particle other than the proton or the electron difficult to conceive. What could such a particle be like? To be fundamental, it too would have to possess *a basic unit of charge*. But the basic units were already possessed by the proton and the electron. Hence, the idea of a third particle, just was the idea of a fundamental material body possessing a basic unit of some third kind of electricity—not positive, not negative, and not neutral, since electrical neutrality was the result of balanced combinations of positive and negative charge. Just as '+' and '−' seemed to exhaust the totality of electrical charge, so 'positive electron' and 'negative electron' seemed to do the same. Since the proton and the electron came to be thought of not only as carrying the charge, but virtually as being the charge,[3] the very conception of a third particle beyond the proton and the electron seemed insupportable.

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[1] Nernst, *Theoretische Chemie*, pp. 197, 456

[2] And still is, of course. Compare Chadwick, Blackett and Occhialini, 'Some Experiments on the Production of Positive Electrons', *Proc. Roy. Soc.* 1934, **144**: 'The name "positron" for a particle of about electronic mass and positive charge was suggested by Anderson, and it seems to be coming into general use. The negative will still be called "as usual" an electron' (p. 235).

[3] Consider Schrödinger's characterisation of the electron as constituting an electrical 'density smear' of interfering electromagnetic waves. *Ann. Phys.* 1926, **79**, 361 and 489. Cf. also Dirac, writing in 1932 (!), where he says: 'The [electromagnetic] field should appear in the [relativistic quantum] theory as something more elementary and fundamental [than the particles].' *Proc. Roy. Soc.*, **136**, 454

It is this profound conceptual resistance, built into the structures of classical electrodynamics and elementary particle theory, which must be appreciated in order to understand why physicists like Dirac, Blackett, Skobeltzyn, Pauli, Oppenheimer, Anderson, Bohr, and Rutherford struggled so hard to avoid having to make such a supposition. Given this, the positive electron hypothesis had to be repugnant to the physicists of 1930. All the more, then, must we admire the conceptual boldness of Anderson and Dirac who, in their different ways, at first tumbled to the conclusion that positive electrons exist, and then had the courage to stand up and be counted amidst a potentially unreceptive scientific audience.[1]

[1] Reactions to the interpretation offered here have not been uniformly favourable. Thus, Oppenheimer writes: 'I, for one, am wholly unaware of the need for any explanation reaching back into history for a firmly held belief that there were only two particles in nature. We were talking about neutrinos and neutrons, and no one could look at the cosmic ray pictures without getting the impression that something was up that could be neither electron nor proton.' [Letter to the author, 12th August 1960.] Niels Bohr writes: '. . . at the time of the discovery . . . physicists were certainly prepared for surprises to a degree far beyond that prevailing in former centuries. Furthermore since quantum theory gives no clues as to which kinds of particles may exist in nature, the question of new particles was from the very beginning realized as completely open.' [Letter to the author, 28th October 1960.] And N. F. Mott writes: 'I would say looking back on it all that one's reluctance to accept Anderson's discovery was due to our lack of realization that the positive electron was created and unstable.' [Letter to the author, 26th August 1960.] Indeed, in light of Rutherford's anticipation of the existence of an elementary nuclear constituent, Bohr feels that 'it is hardly justified to ascribe to him a limited outlook influenced by the early theories of electricity'.

Against such an assembly one can barely imagine a defence. Yet the following remark of Professor Dirac is defence enough: 'I find this [interpretation] very good. It explains pretty closely my feelings at the time and my unwillingness to postulate a new positively charged particle.' [Letter to the author, 1st November 1960.]

With the issue left thus in equipoise, the reader may reach his own conclusions. I would add, however, that the postulation of the neutrino (by Pauli) in 1923, and the discovery of the neutron in 1932, seem not to count against the interpretation being tendered here. Neither particle is charged: our argument is primarily concerned with the possibility of there being a third charged particle, since it would have been commonly assumed that no fundamental particle could be neutral inasmuch as electrical neutrality was felt to result only from the balanced combinations of positive and negative particles. Besides, just citing the dates of the neutrino and the neutron will not by itself destroy my point, namely, that these particles were looked upon as being 'strange', and very difficult to absorb in any existing conceptual framework. Even so, Dr John Ziman rejoins: 'I just don't believe it was as strong as [your interpretation suggests]. . . . All scientists are conservative. They hate to have to think about new elements. . . .' [Letter to the author, 20th October 1960.]

4 *The Blackett-Occhialini Particle*

In a remarkable paper entitled 'Some Photographs of the Tracks of Penetrating Radiation',[1] P. M. S. Blackett and G. P. S. Occhialini swept away all residual resistance to the existence of positrons.[2]

They discovered that the 'Anderson particle' and the 'Dirac particle' were the *same* particle. This is a meta-physical discovery which, in the history of science, ranks high within a select class.

Blackett and Occhialini employed a new experimental method. Whereas Skobeltzyn succeeded in photographing one track in every ten expansions, and Anderson but one in fifty, Blackett and Occhialini arranged to have high speed particles take their own photographs.[3] This new technique succeeded in photographing 80 per cent of all the high speed particles moving through the chamber. This matter of frequency was important to the question already felt so keenly by Anderson and Dirac: 'Are these the tracks of protons?'

Blackett and Occhialini sandwiched the cloud chamber between two Geiger-Müller counters. Any ray passing through both counters must also pass through the illuminated part of the chamber (ibid. p. 717). The time required from the discharge of the counters to the end of the expansion was but 1/100 of a second. Hence the ions diffuse only a short distance from the particle's path. The tracks were only 0·8 mm. thick; this allowed accurate measurements to be made. 1/100 second after expansion an illuminating flash begins; it lasts 1/30 second. 700 photographs revealed 500 high energy particles. 75 per cent of all the photographs disclosed a single particle-track traversing both counters. Within a magnetic field of 2000 Gauss these particles remained undeflected. So their main energy was greater than 300 mev. Other photographs, however, showed multiple tracks, such as had been originally detected by Skobeltzyn.[4] The Blackett and Occhialini photographs reveal an astonishing complexity amongst these secondary tracks. Their interpretation was difficult.

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[1] *Proc. Roy. Soc.*, February 1933, 699 ff.

[2] Henceforth, the designations 'positron' and 'negatron' will designate positively- and negatively-charged electrons respectively. 'Positive electron' and 'negative electron' will designate the two fundamental units of electrical charge. This may avoid some of the confusions of early twentieth-century electron theory.

[3] Skobeltzyn, *C.R. Acad. Sci., Paris*, 1932, **195**, 315; Anderson, *Phys. Rev.*, 1932, **41**, 405; Blackett and Occhialini, *Nature*, 1932, **130**, 363.

[4] Skobeltzyn, *Z.f. Physik*, 1929, **54**, 686; *C.R. Acad. Sci., Paris*, 1932, **194**, 118; Auger and Skobeltzyn, *C.R. Acad. Sci., Paris*, 1929, **189**, 55.

> The most notable feature common to many of these multiple tracks is the occurrence of a group of several tracks diverging, mainly downwards, from some region in the material surrounding the chamber. . . . Sometimes a group of these tracks appear to diverge fairly accurately from a single point. . . . When such a *shower* of particles is seen to have entered the top of . . . the chamber, it is not infrequently found that a subsidiary radiant point occurs in the metal plate . . . across the chamber.
>
> . . . it is necessary to come to the same remarkable conclusion that has already been drawn by Anderson (*Science*, 1932, **76**, 238) from similar photographs. This is that some of the tracks must be due to particle with a positive charge, but whose mass is much less than that of a proton (ibid. p. 703).

The most important measurement in this connection was for Blackett and Occhialini, just as it had been for Anderson, the determination of the track curvature by a magnetic field ($H\rho$). Also, the ionisation density was significant. This depends only on the charge and the velocity, not on the mass. But the velocity of a particle of a given $H\rho$ depends on the particle's mass. Consequently, observing a particle's $H\rho$ and its ionisation allows an accurate determination of its mass, even though the upper limit of the mass cannot be established reliably for any given $H\rho$ range. On the basis of these considerations Anderson and Blackett reached the same conclusion concerning the new positively-charged particle; it had to have a mass 'much less than that of a proton.'

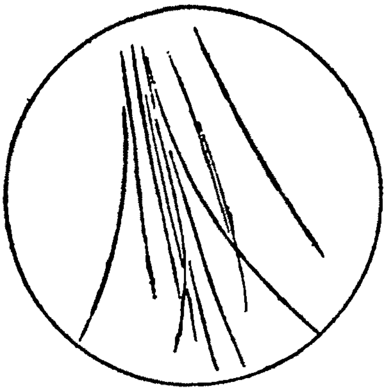

FIG. 5

The 'shower' picture above suggests that all the particles came from the same direction; that any track moving vertically is probably proceeding downward; any particle deflected to the left is negative and any particle deflected to the right is positive. If these particles were protons their ranges could only be 0·2 cm. and 3 cm. in air—but they actually travel 12 cm.! A study of the ionisation density (using Bethe's calculations[1]) confirms this. These particles must be fast electrons; if they were protons their ionisation would be from 10 to 100 times heavier than is actually observed.

> The only possible conclusion of the argument both from the range and from the ionization is that these tracks are due to positively charged particles with a mass comparable with that of an electron rather than with that of a proton (ibid. p. 707).

Blackett and Occhialini found 14 tracks which were identified as positive electrons. Others were less certain. The ratios of the positive and negative particles were about equal in the shower photographs. In similar research Kunze had interpreted all these positively curved tracks as protons. Against this Blackett argues '. . . all the single tracks which do show marked positive curvature have nearly the same specific ionization as the unaffected ones, so are either negative electrons going up, or positive electrons coming down'.[2]

Of signal interest are the following remarks of Blackett and Occhialini:

> . . . at least 7 positive and negative electrons have been found diverging from a single point in glass, copper, and lead, and presumably therefore from single nuclei . . . there are three possible hypotheses . . . about the origin of these particles: (*a*) they may have existed previously in the struck nucleus, (*b*) or they may have existed in the independent particle, (*c*) or they may have been created during the process of collision . . . it is reasonable to adopt the last hypothesis (ibid. p. 712).

One cannot over-estimate the importance of this last conjecture. Not only do these two physicists surely identify positron-negatron pairs,[3] they also give a correct interpretation of them, and even develop a first approximation to what ultimately became (in the works of Oppenheimer, Plesset, Fermi, and Uhlenbeck, 1933) the theory of *pair creation*. It is certain that at about this same time the Joliot-Curies, with their hypothesis of 'materialisation', were encountering similar phenomena. And in the history of physics this is the first time that the hypothesis of creating matter out of energy was seriously entertained, now by several independent investigators simultaneously. This physical discovery in itself sets the positron history apart, and makes it wholly non-comparable to the neutrino and neutron discoveries. Blackett continues:

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[1] Cf. Bethe, *Zeits. f. Phys.*, 1932, **76**, 293; also Carlson and Oppenheimer, *Phys. Rev.*, 1932, **41**, 763.

[2] Concerning terminology, Blackett writes: 'By a positive electron is meant a particle with unit positive electronic charge, and with a mass very much less than that of a proton' (ibid. p. 708). This does not accord with our decision above, p. 305, note 2, and the difference should be borne in mind.

[3] In his letter 'Cosmic-Ray Bursts', Anderson describes 'associated' tracks which are probably positron-negatron pairs. *Phys. Rev.*, 1933, **43**, 368. In *Phys. Rev.*, 1933, **44**, Anderson is 'dead on' target concerning pair-production in cosmic ray showers (p. 416). Cf. *supra*, quotation preceding section 2.

> If, however, the conservation of the electric charge is to be fulfilled, then positive and negative electrons must be produced in equal numbers . . . in this way one can imagine that negative and positive electrons may be born in pairs during the disintegration of light nuclei . . . (ibid. p. 713).

It is at this juncture that Blackett makes his 'meta-physical' discovery, the importance of which was tremendous for subsequent elementary particle theory:

> The existence of positive electrons in these showers raises immediately the question of why they have hitherto eluded observation. It is clear that they can have only a limited life as free particles since they do not appear to be associated with matter under normal conditions.
>
> It is conceivable that they can enter into combination with other elementary particles to form stable nuclei and so cease to be free, but it seems more likely that they disappear by reacting with a negative electron to form two or more quanta.
>
> This latter mechanism is given immediately by Dirac's theory of electrons. (*Proc. Roy. Soc.*, 1930, A, **126**, 360; 1931 A, **133**, f. 60.) In this theory all but a few of the quantum states of negative kinetic energy, which had previously defied physical interpretation, are taken to be filled with negative electrons. The few states which are unoccupied behave like ordinary particles with positive kinetic energy and with a positive charge. Dirac originally wished to identify these 'holes' with protons, but this had to be abandoned when it was found that the holes necessarily have the same mass as negative electrons. It will be a task of immediate importance to determine experimentally the mass of the positive electrons by accurate measurements of their ionization and Hρ. At present it is only possible to say that no difference between the ionization from the tracks of negative and positive electrons of the same Hρ has been detected so that provisionally their masses may be taken as equal.

> On Dirac's theory the positive electrons should only have a short life, since it is easy for a negative electron to jump down into an unoccupied state, so filling up a hole and leading to the simultaneous annihilation of a positive and negative electron, the energy being radiated as two quanta. . . . When the behaviour of the positive electrons has been investigated in more detail, it will be possible to test these predictions of Dirac's theory. There appears to be no evidence as yet against its validity, and in its favour is the fact that it predicts a time of life for the positive electron that is long enough for it to be observed in the cloud chamber but short enough to explain why it had not been discovered by other methods.
>
> It should be possible to find evidence on the photographs of positive electrons which have entered a metal plate but which do not emerge again owing to their annihilation while traversing the metal. It is also possible that the gamma-ray annihilation spectrum may be detectable by observations of the Compton recoil electrons. According to Dirac's theory, this spectrum should have a lower limit at an energy of 0.5×10^6 volts and should extend through a maximum at a slightly greater energy to a steadily decreasing intensity for high energies. . . .
>
> Again the hypotheses of the existence of positive electrons amongst the secondary particle produced by neutrons, would provide an explanation of the curious fact discovered by Curie and Joliot (*Exposée de Physique Théoretique*, 1933, p. 21) that fast electron tracks are found with a curvature indicating a negative electron moving towards the neutron source.

Here is the perfect marriage of experiment and theory. Here we have observations of positrons with understanding. It would be too easy, and too inaccurate, to suggest that Blackett had re-discovered, or simply 'confirmed'[1] Anderson's own work. There is something unique and unprecedented in the Blackett paper. Not only is it a discovery of much that was unknown, it is also a discovery of the fact that some things that were already known are interrelated in unsuspected ways. Of course, Blackett's technique allowed him to observe pair production much more completely than Anderson. The step from Blackett's observations to Dirac's was thus much shorter than for Anderson. Moreover, Dirac's work, at least in principle, was known to Blackett: Anderson was not in the same position to the same degree.

Blackett's paper had a galvanic effect on particle theory. For one thing, as he so candidly testifies himself, Professor Carl Anderson realised that there was a connection between his work and the research of Dirac only *after* reading the paper of Blackett and Occhialini.[1] This must have been true of many others too. For another thing, many ' spurious ' phenomena now seemed amenable to possible re-interpretation.[2]

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[1] Cf. Millikan, op. cit., p. 335

Immediately, searches began for the positron tracks from other sources. In April 1933, Chadwick, Blackett and Occhialini, Curie and Joliot, and Meitner and Phillipp[3] all reported that the bombardment of beryllium by polonium α-rays produces radiation which results in positron-production.

The theoretical work of Fermi and Uhlenbeck (1933) followed directly; the Dirac theory was thus extended so as to give a full understanding of pair creation and annihilation.[4] Moreover, once this important conceptual breakthrough had been achieved, once it was seen to make sense to entertain the possible existence of particles other than the proton and the negatron, elementary particle theory really began to accelerate. Now the neutron, discovered at about the same time Anderson made his discovery,[5] found its first comfortable place within the framework of particle theory. The Yukawa particle was announced in 1935.[6] In 1937 Anderson, again, discovered a particle related to that of Yukawa: these were both what are now called *mesons*. The ground was prepared for the active prosecution of meson theory. Everything in microphysics since 1937 is thus genetically connected, intimately, with the positron discovery of Dirac, Anderson, and Blackett. The importance to all physics of the idea of matter-creation has already been stressed.

Some incidental morals fall gently from this narrative. It is interesting to note that, during the period 1931-32, Dirac and Blackett were working within half a mile of each other. St John's College and the Cavendish Laboratory are but a sparrow's-flight apart. Yet the actual conversation to have passed between these physicists must be adjudged very slight. Perhaps something of the same nature obtained, at the same time, at the Norman Bridge Laboratory. For here, at Cal. Tech., we find not only Anderson and Millikan. Oppenheimer was also at Norman Bridge and later came down from Berkeley. He was intimately familiar with the Dirac theory and must also have known of the exciting tracks being produced within Anderson's machine. Just as the 'Gordon jumps' and the 'spurious tracks' existed side-by-side without being related, so this seemed to be the pattern of positron enquiry until 1933, and Blackett.

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[1] Cf. Blackett, Occhialini, *Phys. Rev.*, **43**, 1034; **44**, 411

[2] For example, the anomalous absorption of γ-radiation by heavy nuclei was now thought (by Blackett) to be connected with the formation of positive electrons and the re-emitted radiation with their disappearance. Cf. Meitner and Hupfield, *Naturwiss.*, 1931, **19**, 775; Chao, *Phys. Rev.*, 1931, **36**, 1519; Gray and Tarrant, *Proc. Roy. Soc.*, 1932, A, **136**, 662

[3] Cf. Chadwick, Blackett, and Occhialini, *Nature*, **131**, 473; Curie and Joliot, *Compt. Rend.*, **196**, 1105; Meitner and Phillipp, *Naturwiss.*, **21**, 286.

[4] Cf. also the important paper by Oppenheimer and Plesset, in which it is shown '. . . in a particularly clear way, that the production of pairs is a typical quantum effect'. *Phys. Rev.* 1933, **44**, 54: the Fermi-Uhlenbeck paper is in the same volume, 510-511.

[5] Cf. Chadwick, *Proc. Roy. Soc.*, A, **136**, 692

[6] Yukawa, *Proc. Physical-Mathematical Soc. of Japan* (3), 1935, **17**, 48, and esp. 57

Of course, there is a sociological dimension to the conceptual history set out here. American theoretical physics was somewhat weak during this entire period. Dirac, and the Germans, would probably not have undertaken to write to Oppenheimer on their own initiative, and certainly not to Anderson. Perhaps such factors affect the growth of science as strongly as does any strictly conceptual or theoretical development.

A practical consequence of the positron-discovery consists in the fact that, since the hypothesis of 'anti-matter' had now seemed fully confirmed, the hunt for the anti-proton[1] and anti-neutron immediately appeared feasible. In fact, the great synchro-cyclotron at Berkeley, was specifically 'built up' to 6 BEV to determine whether or not the anti-matter theory, implicit in Dirac's original paper, could be confirmed for anti-protons and anti-neutrons.

One general conclusion of our inquiry is this:

Those accounts of microphysical discovery which suggest them to be 'single-line' disclosure of facts, reminiscent of the naturalist finding a new bug beneath a rock—such accounts are to be held in suspicion. They distort and dwarf the enterprise of the physicist. But the conceptual complexity within physics has shaped the recent history of human thought. Our accounts of that research must respect that complexity.

Indiana University

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[1] First mentioned by Dirac, *Proc. Roy. Soc.*, 1931, **133**, 62. Indeed, Anderson himself writes (in the original paper): '. . . the discovery of the positron should prove a stimulus to search for evidence of negative protons' (ibid. 494).

SCIENCE AND PSYCHOLOGY *

A. R. Louch

I *Aristotelian and positivist accounts of psychology*

Almost everyone these days agrees that something is wrong with psychology. By and large diagnosticians of these ailments fall into one or the other of two groups. Psychologists, and the positivist minded philosophers from whom they have taken their cue, attribute the difficulties in their discipline to scientific immaturity, a condition which further precisely conducted experiments and the formulation of general laws from which further observed behaviour can be predicted will remedy. I shall call this diagnosis the positivist account.

The other group—mostly philosophers: R. S. Peters's illuminating study, *The Concept of Motivation* is a notable instance—find the trouble, not in the lack of the machinery of general theory, but in the attempts made by psychologists to force their accounts of human behaviour into the logically inappropriate framework of causal hypotheses connected in a deductive system. I shall call this account Aristotelian, for Aristotle's account of explanation was designed to show that different kinds of events and different kinds of questions require different kinds of explanatory moves, thus excluding, on logical grounds, the possibility of an all-embracing general theory like that, say, envisaged by Democritus. In particular Aristotle emphasised the discontinuity of physical happenings and human actions.

I shall argue in the sequel that both sides to this controversy are engaged in an attempt to forecast the future course of psychology, in itself a possibly interesting and harmless pastime, except that these hunches and divinations are reported as if they embodied logical strictures upon the scope of askable questions and usable techniques in describing and explaining human behaviour. This is perhaps now obvious in the case of the positivists' strictures, for the result of imposing procedures appropriate to physics upon the study of human behaviour has been a tendency to play games of theory-construction embarrassingly devoid of useful information about human doings. A good

* Received 7. x. 60

deal of polemical power accrues to the Aristotelian account in virtue of this sterility of much of contemporary psychology. I shall therefore not be principally concerned with the positivist account. The logical strictures proposed by the Aristotelians are vastly more complex and subtle, and it is the move in these accounts from conceptual difficulties within current psychological theories to legislation with regard to any possible psychological theory which will concern me.

The Aristotelian account rests upon obvious differences in the kind of explanatory moves we make with respect to such different occurrences as the movement of billiard balls and the actions of the player of billiards, the trajectory of a bullet and the action of a killer. Contemporary psychologists often appear to override these distinctions between movement and action, occurrence and behaviour, in order to present a theory which has morphological similarities with theories which account for movements and occurrences (such as physical laws) and at the same time preserve a relevance to actions, to human behaviour, to events which require explanations of a different structure altogether. Drive-reduction theories are particularly subject to such criticism. In the effort to satisfy two incompatible claims—relevance to human behaviour and conformity to a limited conception of scientific method—such theories attempt to convert reasons for acting into the causes of movements. They confuse, as Peters very clearly points out, the questions, Why did he do it? and What made him do it? and so they convert motives, reasons, into antecedent impulses bringing about action. A drive theory begins by exhibiting human tendencies to pursue certain goals, such as hunger, thirst, and sexual satisfaction, and by extension the 'thirst' for knowledge, the need for security, the drive for success and so on. At this level, a drive account is simply another way of telling us that men have numerous goals which they pursue in a variety of ways. But drives and needs are then forced to do double-duty: in addition to their purposive rôle, they are offered as causes of human behaviour—they 'drive', they 'push' human beings to action. But, having started out life as purposes, they cannot adjust to their new rôle. They lack an essential character of causes, for they are unobservable; they are not members of a sequence of events. Thus in a drive-reduction theory motives become bogus causes.

Freud's theory is entangled in similar logical difficulties. His most basic theory represents a failure to keep apart two logically distinct kinds of explanation. Our grounds for supposing that men pursue pleasure are justifying grounds, that is, the pursuit of pleasure is an accepted reason for doing a great number of the things we do (though perhaps the scope of such reason-giving varies from time to time and place to place). But Freud in effect attached the plausibility of the reason-giving account to a theory according to which men are *driven* by pleasure; it now provides the push in a mechanical system. By doing so he too provides only a bogus cause which remains in principle unverifiable, and requires a vast metaphorical machinery (the libido boiler and sometimes the dammed river) in order to give it an air of plausibility.

Such is the case, very briefly, for the Aristotelian critique of psychology. Its strength lies in the fact that a good many contemporary psychologists have committed the blunder of mixing purposive and causal explanations. But the question is whether *any* causal or *any* general theory account must fail in the task of explaining human behaviour. It is one thing to say that purposes and causes are logically distinct; quite another to claim that causes and general theories are logically inappropriate to the explanation of events which we ordinarily explain by reference to purposes. It may yet be the case, as the positivists would say, that we simply have not yet found a workable general theory. I shall argue that the Aristotelians have provided a defence for the weaker thesis that, given the distinction between actions and movements, causal (general theory) explanations are logically inappropriate to actions. But they use these arguments as claims for a stronger thesis that it is *logically* inadmissible to view as movements behaviour which we customarily think of as actions. I shall try to show that the stronger thesis does not follow from the weaker, and that, in itself, the stronger thesis represents a presumption to decide on logical or quasi-logical grounds what can only be determined in the course of empiricial investigation.

2 *The explanation-prediction gap*

Aristotelians approach the stronger thesis in several ways. For one thing, they attempt to drive a wedge between the concepts of explanation and prediction which, in the positivist-psychologist programme, tend to become collapsed. If we held, like Hume, that all explanation is causal, then the merger of these concepts might appear plausible. For to ask for the explanation of an event would be to ask always for a circumstance from which the occurrence of the event to be explained could be predicted. But consider this case.

Elaborate observations recorded in tables of sunsets could afford a basis for the prediction of any particular sunset, yet we should not refer to such tables as explaining the regular phenomenon of the sunset. We should need something like a model of a rotating earth with respect to a relatively fixed sun. Moreover, we should be inclined to accept the model as a possible explanation even if it were not constructed in a way accurate enough to allow for the predictions which we can make successfully from our non-explanatory tables. Of course, this much must be said for the positivist account. When we have formulated the earth-sun model with a precision allowing for predictions of the sun's apparent motions we have grounds for adopting the model which we lacked before; we have created possibilities of verification for the theory. But none the less the concepts of explanation and prediction cannot be collapsed.

The gap between explanation and prediction appears even wider when we turn to accounts of human behaviour. Suppose we were to ask: why are some railway crossings always equipped with grids? and someone should answer: there are always grids at open crossings where cattle are pastured. We could then use the occurrence of grids as a means of inferring the presence of cows or at least of unfenced pasture, and conversely. But it would be odd to say that we know now why the grids are there, a question that can only be answered properly by saying: they are put there in order that cattle shall not unduly be reduced to hamburger by passing trains. The explanatory formula adds nothing to the predictive power contained in the observed regularities; in many such cases, the reference to purposes, motives, intentions, reasons, detracts or distracts from the ability to predict, for there are many slips between plans and actions. But reason-giving or purpose-revealing seem none the less to be the only proper explanatory accounts of such situations.

3 *Logical fit*

What allows this logical fit of certain why questions and purpose or reason-giving answers? One is tempted to answer: the way the question is phrased. But while this is a sound case, it is surely a weak one. If we ask, 'Why *did* he do it?' (with some kind of stress on doing) it does seem peculiar to talk immediately about tumours or hidden drives or antecedent events without prefacing these explanatory remarks by saying: 'Look here, he didn't do it at all, such and such made him.' Similarly to 'What made him do it?' a reply telling us about deliberations is not relevant. We can, clearly, state questions in such a way as to demand a purposive account or a causal account. But if this is the Aristotelian position it is a weak one, for surely the question of importance is not, what is the relation between a question and an answer but, why ask this question rather than that? The positivist might still subscribe to his programme, for he could say: we have been trapped by a tendency to frame questions that demand purposive answers. Such answers, however, generally fail to meet the test of confirmability, and we suggest phrasing new questions about human behaviour which would demand other kinds of answers (causes or general theories) which may turn out to be verifiable.

Perhaps the Aristotelian account is contextual then; why questions acquire a particular meaning in the light of kinds of information ordinarily requested with respect to some subject matter. Thus a general theory is appropriate to the question, why do the planets appear to move as they do; a causal account to, why did the lights go out; and a purposive account to, why did he do it? But it would be misleading to regard the context in these cases as grammatical or logical, with the suggestion that the appropriateness of an explanation-type can be shown by the way in which we ordinarily phrase questions or customarily request answers. The context is rather part of empirical knowledge. Questions about 'things' have come to be associated with causal explanations and general theories because, by and large, 'things' have proved explainable by theories having these characteristics. Questions about people, on the other hand, have not been explained very satisfactorily by these or, indeed, any means. But in so far as human action proves explainable, it seems to be by means of purposive accounts. In both cases our talk about appropriateness of an explanation type is an inference from past successes to future performance. Explanations of the causal type have worked with respect to physical objects in the past, hence we are inclined to cast our explanation-seeking questions about physical objects in this form. Explanations of the purposive type may have worked with some degree of adequacy with respect to human behaviour from this point of view. But some degree of success in asking other kinds of questions about human behaviour could, in principle, dislodge purposive explanations from their favoured position. The criteria of success (with which the positivists were especially concerned) are common to all explanation activities, though one case may not meet them as fully as another.

We do expect, for example, that explanations of human behaviour will include talk about human behaviour (which, of course, many contemporary theories of human behaviour do not) and be addressed to problems which trouble us. But we also expect that an explanation will be formulated in such a way as to provide means of testing it (and here, of course, most purposive accounts fail), perhaps also that it will provide grounds for prediction inasmuch as prediction affords the most decisive sort of verifying case. The application of these criteria is a matter of testing out various theories, that is, it is an empirical matter; and statements about explanation-types are generalisations as to the success of groups of theories having certain common morphological characteristics. To criticise causal accounts of human behaviour is thus to show how given theories fail to relate to their subject matter or fail the tests of confirmability; no logical gap has been exhibited which prevents causal theories from accounting for human behaviour.

Perhaps, then, the Aristotelian claim is about the subject matter itself, as if we could say simply: to billiard balls belong causes, to human behaviour purposes, and so on. By their very nature, some kinds of events exclude or embrace explanation types. And this does reflect, certainly, what we often want to say about Skinner boxes and psychological field theories. There is something queer in talking about human behaviour in this way, a queerness which may be due to the fact that we are in the habit of explaining different sorts of events in different ways, but which at a more sophisticated level becomes translated into the language of logical appropriateness and inappropriateness. Modern Aristotelians suggest that there are logical ties between movements and causal accounts, and between actions and reason-giving accounts. Human behaviour consists, not of movements merely, but of actions; hence causal accounts of such behaviour are excluded logically by the descriptions we offer of such behaviour. This contention depends upon the supposition that statements like ' He acted in such and such a way ', ' He did it ' and so on, are neutral descriptions of the events in question. But surely to call a sequence of movements an action is already to offer an explanation of them. We are saying, these events have a reason, embody a plan, a purpose, are entered upon with an end in view. A causal account of these events is not excluded by the description of the events; it is ousted by a competing explanatory hypothesis, and it remains possible that we may revert to a causal account if our purposive explanation fails to meet empirical tests. It is not difficult to decide that action requires purposive accounts; what is difficult is to decide that a sequence of movements is an action. In other words, we have already interpreted and explained human behaviour when we employ the term ' action '; it is thus not surprising that actions demand purposive accounts, they already incorporate them. But Aristotelians have yet to show that sequences of movements typical of human beings *must* be interpreted as actions. They have shown that many theories—Freudian and drive-reduction theories particularly—have failed to observe a distinction between the questions ' why did he? ' and ' what made him? ' where such a distinction is germane to the theories themselves. But these objections cannot be levelled against a theory which does not depend on a purposive account. Skinner, for example, does not begin with a purpose-laden account and distort it until it takes on the guise of a causal theory. He attempts, at least, to provide the most neutral descriptions possible of behaviour (bar pressings, bell-pullings and the like) and of the conditions that lead to it. It may be, of course, that the observations of rats pressing levers do not *in fact* lead to fruitful theories about either all rats or about human nature. But it is not at all clear that such investigations can be dismissed as irrelevant to the explanation of human behaviour on the grounds that they do not provide answers to questions of the why-did-he-do-it? form.

4 *Are reason-giving accounts logically or empirically appropriate to human behaviour?*

In *The Concept of Motivation* (pp. 13 ff) Peters's argument that only reasons can serve as the sufficient conditions of the explanation of actions depends also upon having assumed that movements characteristic of human beings must be interpreted as actions. For consider his contract-signing case. It does seem absurd to search about for particular neural processes which may have triggered off the signature on the contract, though it makes some sense to talk about neural processes being necessary conditions of the writing of such a signature. ' To obtain the land ' may be the favoured sufficient condition and the explanation for the signing of the contract, but it is favoured on empirical grounds, in the sense that we are led to cast our question in a form eliciting a purposive account because purposive accounts have generally proved workable in such contexts. And this leaves open the possibility that, recasting the question, physiological events might provide the sufficient conditions for occurrences we now call actions. We might doubt the probability of such a move, but we do not have grounds for excluding the possibility.

Consider a rather different case. It would be odd, nowadays, to re-open the question as to why the planets move the way they do, especially if one were to say: we have, after all, only taken into account initial conditions, laws of heavy bodies in motion, and a conjectured model of such bodies moving in a regular way; we haven't talked about purposes. Would there be a logical objection to talking about the purposes that planetary motions serve? Or would we prefer to say: such talk can't be excluded, we can only point to the success of the theory we've got in explaining the motions of those celestial objects it sets out to explain, in predicting their further behaviour? If we deny the teleologist the possibility of a foothold, we seem to be backing up our own theory in a rather extraordinary way. We forget that our theory was arrived at by a long process in which certain accounts were discarded because they proved worthless against the tests of observation and experiment, and suggest now that these experiments after all did not have to be made: for some kinds of explanation are logically inappropriate. The mechanical model has won its spurs, not by a logical fit, which one could discover by an examination of the ways in which the concepts of theories and descriptions are used, but by a method of trial and error. If we now pay little attention to a teleological explanation of nature it is because of the success of the mechanical theory and the failure of teleological accounts to offer in some way or another statements verifiable in experience.

Aristotle's rigid logical rubrics had the effect of cutting off interesting questions about nature as logically inappropriate where they in fact only flew in the face of ordinary opinion. Modern Aristotelians are in danger of doing the same by insisting upon the logical character of a critique of psychology whose force rests in fact upon the ubiquity of particular explanation-types in accounting for human behaviour. Our ordinary talk about human behaviour embodies a theory, but because it is commonplace we are apt to assign it a logical rôle which disguises its theoretical character. We are led to think that human behaviour must be purposive because it consists of actions, forgetting that to use the term action, to use the active rather than the passive voice, is already to explain (and not always with conspicuous success) human behaviour, and not merely to describe it. One might almost say: familiarity with explanation breeds description. Reason-givings, enshrined in ordinary use, lead us to think of alternative, say physiological, accounts, as odd and far-fetched, much in the way Democritus' atomic theory appeared to Aristotle. Thus we are apt to construe the oddity as logical, and close off possible avenues of research, possible discoveries, on *a priori* grounds. But the physiologist or the behavioural psychologist, by asking different questions, and breaking with our common use, lead us to revamp our ways of looking at and talking about human behaviour.

5 *The preference for the causal-mechanical model*

It is worth examining the tendency of psychologists—as witness those psychological theories which come under such devastating attack from Peters—to try out mechanical models (though at present these amount to little more than games), and to convert purposes into causes. Wittgenstein would say perhaps that this is symptomatic of the bewitchment that mechanical models have for us. But might not there be genuine grounds for the preference for the mechanical or causal model *in every case*? Consider the following case. A man is diagnosed as schizophrenic upon admission to a hospital because of symptoms of withdrawal, systematic delusions, and so forth. His family offers the account: 'He's been depressed, very jealous of his wife, has been reading the Bible and thinking about religion a lot.' The psychiatrist talks about the use the patient is making of his symptoms to ward off anxiety, or to assure himself that he is not homosexual, or whatever the psychiatrist's preferred account may happen to be. These accounts are of roughly the same logical type, that is, seeing the patient's behaviour as intended to do some particular job, as a set of reactions to his view of the world. Now suppose the patient is given extensive neurological tests, which eventually reveal a tumour of the brain. Reason-givings, no matter how well they fit the patient's behaviour are dropped in favour of the physiological, causal, account.

The Aristotelians would now say: the relevance of the causal account is due to the extraordinary nature of the individual's behaviour; it is precisely because reason-giving explanations fail that we resort to a causal account. The implication is that causal accounts can only be relevant when the convention breaks down; hence physiology has certain limits beyond which it must not trespass, these being marked by the presence of workable convention accounts. But what is the sign that the convention account breaks down? The Aristotelians suggest that some behaviour is clearly and markedly non-conventional, like the behaviour of lunatics and drunken or drugged men. Such behaviour is, we say, pointless, meaningless, signifying that we are incapable of explaining it along conventional lines. But just as often the convention-account is discarded, not because it breaks down (for we can give such accounts of, for example, the schizophrenic's bizarre behaviour) but because we have discovered a cause of that behaviour in the organism of the afflicted individual. The causal account sometimes ousts the reason-giving account, and does not merely wait upon the occasion of its failure to explain.

The physiologist might thus feel entitled to pursue his enquiries on a broader scale, supplanting other reason-giving accounts with descriptions of neural processes or blood chemistry or endocrine secretions, and thus aspire to provide a general theory of human behaviour. Would he then be trespassing upon someone else's logical preserve? If he phrased his theory merely as an attempt to account for all behaviour in terms of neural processes he would be in logical hot water. For he would not have specified alterations in behaviour with which to connect changes in neural activity. Such is the error of so many blanket theories in psychology; they try to programme the theory before they are aware of what it is that needs explaining. But there could be nothing logically amiss if he were to extend his theory by means of correlations of neural or endocrine activity with specific personality differences say, or intelligence levels, or whatever distinctions in human behaviour (and this, surely, is logically unbounded) he finds worth examining.

The interesting point is the unqualified preference for the causal account over the reason-giving account. Such preference would indicate that there might be something more to the positivist view than the Aristotelians would allow. For the positivists, and following them, most experimental psychologists, have laid stress on the means by which a theory may prove its superiority over its competitors. As a result they have concentrated on conditions which seem to prevail in physics, and have attempted to apply these techniques wholesale to the study of man without sufficient adjustment to peculiarities of the new subject-matter or to the kinds of questions that trouble us. Still, the positivists have laid down the conditions which would appear to be paradigmatic of a good explanation: prediction—not because to predict *is* to explain, but because predictions afford confirming tests for a theory; a hypothetico-deductive theory—because in this way verifiable statements can be inferred from the theory, and the logical course of our inferences from theory to description can be clearly traced; and tinkering—because we often want to be able to alter conditions in order to come more decisively at the diagnostic factors among events. These interests, these conditions, point to causal accounts, to general theoretical accounts rather than to reason-giving accounts as the proper tools for a scientific explanation of any subject matter. They point away from psychology in the narrow sense to physiology. Psychology does not thereby reduce to physics, nor motives to causes. Rather, we choose causal models, we describe human behaviour in terms of movements rather than actions, because such models and procedures stand the best chance we know of verification or falsification.

6 *Verification difficulties of reason-giving accounts*

The Aristotelian account ignores these verification difficulties in conventional reason-giving accounts. We ask: ' Why did John go into the tobacconists? ' and accept the answer ' To buy tobacco '. But our reasons for accepting such an account cannot be generalised for all similar cases. We might give as reasons: John said so, or, he came out with a tin of tobacco. But in other cases what John said is no longer reliable evidence, and what he comes out with is not a sure indication of the aim of his activity. Men marry and take up professions and hobbies and wear particular kinds of clothes; some of them drink and smoke, others do not; some attend church, others listen to ball games or read *The Times* on Sunday; some are devoted to their families, others to their bowling, others to their mistresses . . . in a way all conventional behaviour because common behaviour, but actions for which rules are not forthcoming and for which goals are not unambiguously defined. The tobacco-buying incident is thus misleading as a paradigm of human behaviour. In this class, perhaps, belong going to a movie or a ball game, or, at least, buying the ticket. If asked ' why ' on such occasions we say, ' because he wanted to ', ' beacuse he likes it ', sometimes ' because it's the thing to do ', ' because that's the way it's done '. But often such accounts are deliberately *ad hoc* and specious. They are ways less of answering the question than of shrugging it off, and the questioner, more than likely, accepts them because his question was asked idly anyway. If you accept the account ' To buy tobacco ' on the grounds that ' John said so ' or ' He came out with a tin of tobacco ' or ' That is why I would have gone into a tobacconist' it is because you aren't particularly concerned about this case, conviction can be bought easily. But when someone tells you why he married or divorced the person he did, or why he wants to become a millionaire, and you are really concerned, puzzled, by his behaviour, his words won't satisfy any longer, the consequences of his actions no longer have the tin-of-tobacco-in-hand clarity, and it only muddles the issue to put yourself in his position.

Both the Aristotelian and the positivist accounts are vague here, in that they both carry the suggestion that a general theory must somehow incorporate John-buying-tobacco as a special case of the theory, and thus oust reason-givings. But a general theory, considered simply as a set of hypotheses from which particular descriptions of behaviour follow, may or may not explain the tobacco purchase; there are no considerations by which we are able to insist that the tobacco buying is germane to the theory, any more than all the vagaries of heat convection in one's furnace are to be predicted by the laws of thermodynamics. There is scope left in both cases to piecemeal and *ad hoc* explanations, which turn in the furnace case on how to fix it and in so many human actions on justifying it. But the Aristotelians, as it were, say to you when you raise the suggestion of causal hypotheses or general theories in psychology, 'you mean to say you don't think that John went to the tobacconist to buy tobacco?' 'Well,' you say in some embarrassment 'Not quite that.' And of course it's not that at all. You wish to point rather to those many cases where reason-givings are tenuous, and where they seem to break down altogether. The tobacco buying case is thus misleading, for it focuses attention on an aspect of human behaviour which does not have as wide a provenance as the use of the example suggests, and which leads us to say, prematurely, that there are radical, logical, differences between human and physical behaviour.

It is in the same way misleading to say as Peters does (p. 7) that 'Man in Society is a chess player writ large'. The aphorism suggests that most of human behaviour is ceremonial. Ceremonies are not particularly troublesome; we can describe the movements which make them up, we can look up the rules for their performance in rule books or ask someone designated as an official of the ceremony—priest or umpire or president or chairman of the board. More often than not we are our own referees in the performance of ceremonies of living. Thus we seem to have restored to 'common sense' the explanation and exploration of human actions which have been usurped by professional scientists, statisticians, and field-workers. But the professional scientists have not had as their aim, or need not have had as their aim, the replacement, in every area, of ceremonial accounts with causes or general theories. Their concern has been with large areas in which ceremonial accounts have failed; for example, with the failure of classical economics to account for economic phenomena by means of a ceremonial description of money behaviour; with the nature and origin and change of ceremonies, and with that wide range of responses which a man has at his disposal within most ceremonies. The chess game gives the picture of a life that can be lived almost entirely according to rule; perhaps a better analogy would be to a game like baseball, or boxing, in which some part of the participants' actions can be called ceremonial but much of it requires explaining by reference to other considerations. For in the carrying out of the ceremony itself, factors which are not to be found in the rule book exercise a decisive influence. We are not usually able to predict from the rules alone how a person will behave, and to suggest we can is to select a narrow range of human actions which happen to have a chess-like quality, and regard these as representative of human behaviour. Once out of that range we discover our reason-giving accounts must be applied much more tentatively. There are no verifying procedures whereby conflicting reasons for an action can be decisively adjudicated. Often, competing reason-giving accounts fit the evidence equally well, but further moves allowing a choice among them are not forthcoming. In the absence of possible verifying tests for explanatory moves of the reason-giving sort, psychologists have naturally turned toward causal accounts, toward the conception of a general theory, from which, they hope, verifiable statements can be inferred.

7 *Conclusion. Logical difficulties* IN *theories and logical difficulties* OF *theories*

No doubt, logical difficulties, which, presumably, it is the function of philosophers to expose, plague most psychological theories. Freud's theories are stated in such a way as to be immune from falsification. Concepts like reaction formation are introduced to convert negative into supporting evidence. Lewin's field theory consists almost entirely of definitions, treated as if they were hypotheses of a general theory. His account contains a description of all the moves that ought to be made in a general theory, but he, in fact, makes none of them. Drive-reduction theories, as Peters has shown, use motives as bogus causes. But these logical defects, which of course require much more adequate statement and defence, arise *within* the formulation of a theory, they are not symptomatic of an initial trespass of an explanation-type on to a subject matter logically inappropriate to it.

It is not my intention to endorse the general theory programme of psychology or the substitution of physiological for psychological explanations of human behaviour. It is only my contention that the success or failure of such an enterprise requires experimentation and cannot be discovered wholly by logical analysis. Philosophers, it seems to me, have erred in two ways. The Aristotelians have supposed that because we commonly talk about 'human actions' and because the logic of 'action' can be shown to entail reason-giving explanations, we are committed to looking at human behaviour as sequences of actions rather than movements. The positivists have tried to force a form of explanation on every subject matter without sufficient sensitivity to the different questions we may ask or the different requirements a subject matter may impose on us. But the positivists have pointed to a feature of the business of explaining which the Aristotelians have tended to ignore. We can not only describe differences in explanatory moves, we can assess these moves in accordance with canons like coherence and confirmability. The Aristotelian description of the 'geography' of explanatory concepts conceals also an evaluative element which is not entailed by the description. When the term logic appears in a description, it suggests that the arguments are not only thus and so, but that it is right, fitting, appropriate, for them to be thus and so. Thus in addition to the geography of explanation, the Aristotelians have provided, as it were, the immigration rules to the different boundaries they have explored. It is not just that we do look at human behaviour as a sequence of actions, but that it is proper for us to do so, and improper to attempt to look at it in any other way. Now there may never be the occasion or the need to decide whether these immigration rules are to be enforced or abandoned, for a general theory of human behaviour of any cogency may not be forthcoming. But for a philosopher to claim that the search for one is logically misconceived would be presumptuous.

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DISCUSSION

A CRITICISM OF POPPER'S THEORY OF SIMPLICITY

I WISH to point out an inadequacy in Popper's theory of simplicity by discussing two cases. In the first example Popper's criteria of falsifiability for measuring simplicity lead to a counter-intuitive result immediately. In the second example we have to consider the criterion of invariance, and are led to the conclusion that the transformations which might distinguish between the two theories to be compared are 'irrelevant'. This again leads to a counter-intuitive assignment of simplicities.

I

One of the main attractions of Popper's approach to such problems as defining the simplicity of a theory is, to my mind, that his criteria are, up to a point, 'intrinsic'.

I shall call a criterion 'intrinsic'[1] if its application involves only the object to be judged (in this case the theory, or, in the case of a relative measurement, the two or more theories to be compared) in the way of required data, i.e. if the judgment is based entirely on internal evidence.

It is, in my opinion, a 'good thing' to measure simplicity of theories 'intrinsically' as far as possible, if only to see just how far one can go.[2] Moreover, non-intrinsic measurements of simplicity, such as those that take into account what may be known to the author of the theory at a given date, may lead to pragmatic difficulties, if not paradoxes ('was it known to the author at the time to what tests his theory would be put?'), and their acceptance makes it difficult to draw a line to exclude *any* criteria, such as the prevailing political mood influencing the author writing the theory, or, perhaps later, the critic reading the theory.

Now one of the great achievements of Professor Popper's *The Logic of Scientific Discovery* is that it provides a relative measure of simplicity that is intrinsic up to a point; but, curiously, it is not to be kept intrinsic all the way: we can, under a wide range of conditions, compare two theories and decide which is simpler by a laid-down procedure. But, unfortunately, this procedure has a leak: a theory whose mathematical expression involves few terms (that is, a theory intuitively considered simple) will, by this procedure of measurement, be given equal merit with a theory involving many additional parameters which have artificially been given special values adjusted *ad hoc* to fit the available data.

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[1] I was tempted to use the word 'objective' instead of intrinsic, but found that this carried unwanted overtones.

[2] Just as it is of interest to see what machines can do, in order to define more closely the little difference' between them and human beings.

Consider the following example: we are given experimental data showing the dependance of y, the refractive index of the material of a rod, on x, the longitudinal strain. Suppose that, according to theory A, the relationship should be linear

$$y = 1{\cdot}50 - 2x$$

whilst, according to theory B

$$y = 1{\cdot}50 - 2x + {\cdot}03x^2$$

Judged by Popper's falsifiability criteria, the two theories are equally simple (since each is falsifiable by a single determination). This is clearly counter-intuitive.

Whilst we are not required to consider the question of agreement with facts when considering simplicity, we may assume the requirements of agreement with fact, and of logical consistency, to be met by all theories under consideration. Thus we may take the special case where the two theories fit each point, found experimentally, within the estimated probable error. We might even allow theory B to fit, if anything, more closely than theory A. Indeed, theory B may have been formulated *after* the operation of least-square fitting, i.e. is an *ad hoc* theory. The assignment of equal simplicity to the two theories is, then, undesirable for the additional reason that it lands us in the following dilemma: Horn I: we would be forced in every given situation of accumulated knowledge to allow a class of theories obtained in a routine way by *ad hoc* fitting to the data to be as meritorious[1] as, or even more meritorious than, prior predictive theories. Popper's criteria would allow a class of theories obtainable in a routine way by *ad hoc* fitting to the data to 'leak in'.

Of course, *no* intrinsic measure could exclude *ad hoc* theories as such. The objection to Popper's explicatum of simplicity is that it automatically opens the door to a class of worthless theories obtained by a routine trick: a theory of this class incorporates all the available data, and perhaps accepted theories, and adds an arbitrary law for prediction of the results of certain new experiments to be carried out in the future. This law is highly falsifiable and therefore very simple. Since the theory is, in addition, in agreement with previous results, it would thus rank as highly worthy of refutation.

We clearly want a measure of simplicity that discriminates between theories A and B, and would be a challenge to theories however obtained, to interpret the available data in an effective way.

Horn II of the dilemma would consist in our abandoning the intrinsic nature of our simplicity criterion. We have already discussed the disadvantages accompanying such a policy.

In the cases considered the intuitively simpler theory does *not* 'tell us more'[2], and hence we cannot use Popper's criteria (essentially measuring content) to establish its greater simplicity. We conclude that (relative) measurement of the empirical content of theories is not enough. If we wish to avoid criteria extrinsic to the theory, we must consider not only 'semantic' simplicity (as Popper does) but 'linguistic simplicity'[3] as well, in order to stop the leak.

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[1] The aim, after all, is to arrive at a definition of simplicity that would serve as an intrinsic figure-of-merit for theories.

[2] K. R. Popper, *The Logic of Scientific Discovery*, London, 1959, p. 142

[3] I have used these terms in 'Simplicity in Scientific Theories', this *Journal*, 1960, **11**, where I show the two types of simplicity to be complementary (p. 36). They have to be used jointly.

2

The problem of comparing the simplicity of two theories, following Popper's prescriptions, becomes more complex if those theories involve free parameters, which are not assigned fixed numerical values by the theories.

For example, let us retain our interpretation of x and y, and compare the two laws

(A^1): $y = ax + b$

(B^1): $y = ax + b + \cdot 03x^2$

where a and b are free parameters.

Here the 'dimensionality' (essentially related to the number of determinations necessary to falsify a law) of the two laws is again the same, but now we also have to consider their form-invariance under various transformations.

It is indeed true that invariance under a larger class of transformations in general implies greater falsifiability, and hence greater simplicity.

In any case, the question of invariance of laws is interesting and important (it would be interesting to grade types of laws according to degrees of invariance, from the mere requirement of 'balanced' dimensions to laws involving only dimensionless quantities restricted to integral values[1]). For this reason, I will explain why I do not think that Popper's theory of simplicity can be rescued in this case by an appeal to invariance (such an appeal would not, in any case, apply to theories A, B, which are equally lacking in invariance under transformations).

Whilst it is not entirely clear how we are to proceed in measuring simplicity in the light of conflicting criteria (e.g. when comparing $y = ax + b$ with $y = 3x^3$, the former favoured by the invariance criterion, the latter by the criterion of low dimensionality), it is at least clear that, in the above example, the 'dimensionality' of A$'$ and B$'$ is the same.

On the other hand, $y = ax + b$ is form-invariant under the transformation $x \rightarrow \alpha x + \beta$, whilst $y = ax + b + \cdot 03x^2$ is not, and such invariance does, in general, imply greater falsifiability.

However, Popper dismisses as 'irrelevant' e.g. transformations involving rotation of the co-ordinates in cases such as these (loc. cit. p. 143, 134).[2] Now, is the transformation $x \rightarrow \alpha x + \beta$ 'relevant'? This question can only be settled by translating 'relevance' into an explicit criterion.

A theory that is not invariant to a '*relevant*' transformation $x \rightarrow \alpha x + \beta$ implies a *physical* difference associated with different values of α, β. To falsify such a theory requires not only determination of a minimum number of x, y pairs (one less than the number of 'dimensions'), but also, in general, specification of α and β, measured relative to some, usually 'preferred', co-ordinate x.

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[1] We are here referring, of course, to the 'dimensions of a quantity' in the sense customary in physics. 'Dimensions of a law' refers to Popper's use of the term, as explained in Appendix I of *The Logic of Scientific Discovery.*

[2] It would lead too far here to do more than state that I have reservations towards any philosophic theory, such as Popper's, which gives a special status to descriptions of space-time location, or is based on distinguishing in principle between laws and boundary conditions. (*The Logic of Scientific Discovery*, §§28, 80)

In this case, it seems to me, the transformation is '*irrelevant*', since potentially falsifying *either* theory involves nothing beyond the determination of three values of the refractive index for three values of strain. Thus we would still be forced to regard A′ and B′ as equally simple.

On the other hand, in the absence of an explicit criterion of relevance, we may either dismiss the following transformation as 'irrelevant' in gauging simplicity, or we may in fact transform to a new variable $y^1 \equiv y - \cdot 03x^2$, which would make expression B′ identical in form with A′ and therefore equally simple.

The moral to be drawn in both cases (A, B and A′, B′) is that semantic simplicity is not to be isolated from linguistic simplicity. Consideration of linguistic simplicity presumably requires recognition of a preferred basic language. This step is not, perhaps, as objectionable as it seems at first sight, because such a language may, in turn, be preferred on operational grounds.

H. R. Post

REVIEWS

Lincos: Design of a language for cosmic intercourse. Part I. By Hans Freudenthal.
North Holland Publishing Company, Amsterdam, 1960. Pp. 224. 45s.

THE only reason why I do not give my logic lectures after the fashion of Lincos is that it would take too long. I would like to start by displaying the primitive symbols in silence, the class would recognise the abstract shape that it is meant to convey (no two x's are exactly alike as Russell once said), then give numerous examples of formulae, crossing out those that were ill-formed. Then a display of axioms, then a display of rules of procedure. All this done in silence. The class would recognise the formation rules and the procedure rules just as an ordinary mathematician recognises a proof though he is never explicitly told what a proof is. When one has introduced arithmetic then one can use Gödel numbers. This would be preceded by numerous examples of finding the Gödel numbers of well-formed formulae and of sequences of well-formed formulae. Definitions, merely for purposes of abbreviation, would be required and this could be explained by numerous examples. Having got as far as this, one can put down, again in silence, statements about the system in the system itself. Certain concepts, such as arithmetical truth would require bound class variables and if these variables had not been introduced then they and the rules for using them could be explained, in silence, by numerous examples. And so on. The reason I should like to do this is because a formal system, of say arithmetic, should be independent of any metalanguage. (Cf. Lorenzen's approach in *Einfuhrung in die operative Mathematik und Logik.*) It is in this manner that the teaching of Lincos to celestial beings proceeds, except that instead of symbols we can only use radio signals of different wavelength and duration, so that Lincos could be translated into a musical score or Lincos words could be represented by ordered pair of numbers, the first giving the wavelength and the second giving the duration, and the pauses between words (corresponding to punctuation in an ordinary language or to brackets in formal languages) would be represented by one number giving the length of the pause. The length of the pause giving the order of breaking down a sentence, it would break first at the greatest pause. Thus all the text-programmes in this book could be represented by numbers in this way and it would be interesting to do this and then give it to a panel of decoders to decipher.

An ordinary conversational language is taught to a child in the first place by demonstration. To teach a language to a celestial being the only things we can show are radio signals so the problem is how to begin and make any being who picks up the signals realise that we are trying to teach a language in which we can communicate to them and they to us. The idea is first to try and get over something that we might think to be universal, such as ' twice two is four ' or ' God is Love '. This book starts with the former, the latter may appear in Part II because the author wishes to cover the whole range of human behaviour. There appear to be only two things that we can show by means of radio signals, the first is the natural numbers, 1, 2, 3, . . . and the second is the time interval of a second. The book then starts with the ' ostensive numerals ': these are sent out as ' peeps ' namely: peep——peep, peep,——peep, peep, peep——and so on, the commas denote short intervals, the horizontal lines denote longer intervals. It is hoped that any celestial being who picks these up will realise that they are sent out by other beings and do not belong to the general background of radio waves in the universe. Ostensive numerals are then replaced by algorithmic numerals in the scale of 2: peep $= 1$; peep, peep $= 10$ etc.; where '$=$,' '1,' '0' are radio signals of various wavelength and duration. Having got this idea over we can then teach elementary arithmetic by numerous examples of addition, multiplication, etc., for example $10 = 10$; $11 = 11$; $10 < 11$; $10 + 11 = 101$; $10 \times 11 = 110$, etc., (in the scale of 2), where '$=$' '$<$' '$+$' '$\times$' would be radio signals of different wavelengths and durations. One then hopes that any celestial being who picks up the signals would realise what we are trying to say. Then would follow the connectives of the Propositional Calculus, again given by numerous examples, such as $10 \times 10 = 100 . \rightarrow . 100 = 10 \times 10$, $10 \times 10 = 100 . \rightarrow : 10 \times 10 = 100 . \vee . 10 = 100$, and so on; in this way we would teach elementary tautologies. Free variables can be introduced as follows:

$$\begin{gathered} 100 + 111 = 111 + 100 \\ 100 + 11 = 11 + 100 \\ \text{etc.} \\ 100 + a = a + 100 \\ \text{etc.} \\ b + a = a + b. \end{gathered}$$

Sets are introduced by numerous examples of membership of the set, for example $1 \in \text{Num}$; $10 \in \text{Num}$; $11 \in \text{Num}$; etc. The universal quantifiers by numerous examples, for example, $a = 1 . \rightarrow . a^{10} > 0$; $a = 10 . \rightarrow . a^{10} > 0$, etc. then $\wedge a : a \in \text{Num} . \rightarrow . a^{10} > 0$. The dots denote pauses, the greater number of dots the longer the pause. True and false for arithmetic statements are introduced by numerous examples, for example, $1 + 1 = 10 . \in \text{Ver}$; $1 + 1 = 11 . \in \text{Fal}$, and so on. $p \wedge q . \in \text{Ver} : \rightarrow : p \in \text{Ver} . \wedge . q \in \text{Ver}$, and so on. Each new symbol is replaced by a new radio signal. In this manner there seems no limit to the amount of mathematics one can send out and which an intelligent celestial being should be able to decode. Recursive function theory, Turing machines, etc. are not mentioned but would go over very easily.

The time interval of one second is shown as follows:

$\text{Dur}\overline{\qquad} = \text{Sec } 1$, $\text{Dur}\overline{\qquad\qquad} = \text{Sec } 10$ etc., the horizontal line representing a signal on a new wavelength of duration, 1 sec, 2 sec, etc. Dur and Sec are other radio signals differing in wavelength or duration or both to any yet sent out. When this has been made clear thereafter a clock ticks seconds throughout any programme.

A new quantifier turning a statement into a question is introduced $?\, x : x \in \text{Num.} \wedge .\, x^{10} = 10.$

(Is there a natural number whose square is 2?)

Though this quantifier does not appear in formal languages, there is a vestige of it in some systems which put a sign $\vdash$ (or something similar) in front of a theorem and a statement without such a sign preceding it is put up for consideration. But in these cases '$\vdash$' does not belong to the formal system.

The author wants to communicate all human behaviour to celestial beings and he starts by numerous examples of conversations, for example

Ha Inq Hb $?\, x \,.\, x = 10 + 10$
Hb Inq Ha $10 + 10$
Ha Inq Hb Mal
Hb Inq Ha 100
Ha Inq Hb Ben.

By numerous such examples one hopes that a celestial receiver will realise that Ha and Hb represent beings and that Ha Inq Hb p means Ha says p to Hb, that 'Mal' means 'bad' and 'Ben' means 'good'. And so on until Ha and Hb are playing simple games and discussing whether the game is fair, thus introducing a little elementary probability.

The last chapter deals with space, motion, and mass. The centimetre is explained in terms of the velocity of light and more accurately by Rydberg's constant which occurs in a description of the Hydrogen spectrum.

Mass is tentatively introduced by Ha wanting Hb to pass him something, by carrying it (Fer) or throwing it (Iac), which Hb says he cannot do because though its volume is not too big, its 'Mas' is too big. Collisions are dealt with. Ha and Hb play ball. It is hoped that a celestial receiver will realise that the signal 'Mas' means 'Mass'. The gramme is introduced later on via the universal gravitation constant. The book ends with a few talks on relativity.

In the chapter on behaviour Ha and Hb do not cheat when they play games, but they do play for money, the unit of which is not explained neither do they throw each other about. But Hb does interrupt Ha at one place and this gives an opportunity to send signals meaning polite, etc., for example, Ha Inq Hb: ? x . t_1 t_2 Fit

Hb Inq Ha: A

Ha Inq Hb x (to complete his sentence ? x . t_1t_2 Fit x)

Ha asks Hb what happened between times t_1 and t_2.

Ha Inq Hb:—Dec: Hb Inq Ha A. Ant . Ha Inq Hb x

(not decent that Hb says A to Ha before Ha says x to Hb).

Practically all the text programmes are easy to read except those on Modalities and on Relativity, the reason is that three letter abbreviations of common Latin words are used for Lincos words instead of an ordered pair of numbers representing wavelength and duration respectively. One can make it out easily only glancing now and again at the punctuation. At the end of the book there is a register of Lincos words, 300 to 350 of them, but one rarely needs to refer to it because the three letter abbreviation is usually sufficiently graphic, for example ' Ver Tan Mal ' obviously means ' true but bad (answer) ' like an answer log 1 instead of 0.

Lincos is moderately formalised. I should prefer complete formalisation in the mathematical parts at least until we got into contact with some celestial being and then we might be less severe if it could send back ' —Itg ' (I don't understand). It is of no interest to us to send out radio signals unless we get a reply even though it was received and understood. The chance of getting a reply in a reasonable time seems remote, the nearest intelligent being may be many light-years away, possibly hundreds of light-years away. Again we do not know how long a civilisation lasts, a species lasts say a few million years on this planet, and this is a very short time to the universe so that civilisations on different celestial bodies might fail to overlap. For instance, there might have been intelligent beings on Mars 100,000,000 years ago and since then extinct by the gradual loss of atmosphere.

Any being who received and understood our messages and who could reply, would have to be at our state of knowledge or much in advance of it. There might be beings reasonably near, a few light-years away, still in the neolithic stage and we should have to wait thousands of years before they could reply. In all we might send out messages for thousands of years before getting a reply and might get tired of doing so long before this period was up.

There might be beings 10,000 years in advance of us living in an Arcadia, perhaps of their own making, and who have lived in this happy and contented state for so long that any trace or memory of a former hard existence (such as ours) had completely disappeared. Such beings may long ago have settled problems that still trouble us, for example Goldbach's conjecture, life, consciousness, etc., and it would be of tremendous value to us if they could communicate their theories to us. They might have an advanced theory of unnatural numbers (non-standard models), elementary particles, etc. Their whole history of mental development might have been quite different from ours, for example, *tertium non datur* might be deeply ingrained from an early date, their Aristotle might have discovered relations instead of leaving it to their de Morgan and Pierce, so they might have an elaborate theory of relations while we have little, and this might be fundamental in their Science.

Anyway, it seems perfectly clear that we can communicate all mathematics and a great deal of Science to celestial beings in our state of knowledge or more advanced. And if we did no more than try to communicate these matters and get into contact then, the value to us would be enormous.

If there are intelligent beings on some other celestial body then there might be utterly fantastic creatures possibly capable of receiving radio waves and converting them into sense data the same as we do with light, possibly they might be able to emit radio waves (some creatures on this planet emit light), they might be insect-like, bird-like, fish-like, plant-like, they might be of vast size and so on.

Just as their bodies might be fantastic, so might their language. But if they are intelligent creatures, then surely they must be acquainted with the natural numbers. They might require no banking, or cooking, or trade or building or surveying, etc., but if they can perceive the present falling into the past and if they have a memory, then according to Brouwer they have all that is necessary for them to develop mathematics. If they are going to perceive a series of peeps, then they must realise that a present peep gives way to a peep in the past and is replaced by a peep in the present. For us, perceiving our heart beats falling from the present into the past and memory is all we require to develop mathematics.

A language is required solely for purposes of communication and to assist the memory by making permanent notes. Possibly our memory is not sufficiently strong to enable us to develop mathematics without making permanent notes. Thus it might be that there are intelligent beings far in advance of us mathematically and who have no language, for instance, huge fish-like creatures with a vast memory organ swimming about in a sea of food and without any limbs or other means of making notes (or of anything to make them on), capable of receiving and emitting radio waves. These creatures might be without any form of language and we should have to teach them one. Hence a fully formalised mathematical language would be the most appropriate.

Again they might have a highly developed language capable of expressing a great deal of mathematics but quite different from conversational languages and from the common kinds of formalised languages. For instance, it seems that any formalised language which we know can be modified so as to be formed by application and abstraction only, each primitive symbol is given a type and the rules of formation of well-formed formulae by application and abstraction are given by reference to type and the formula itself is given a type, for example $(\phi_{(\alpha\beta)}\,\psi_\beta)$ is of type α, $(\lambda\xi_\beta\,\phi_\alpha\{\xi_\beta\})$ is of type $(\alpha\beta)$. Here we have punctuation (brackets) and variables (ξ_β). But Łukasiewicz uses only applications in which case brackets are unnecessary (omit the brackets from a well-formed formula formed by application alone and there is only one way of putting them back so that the result is well-formed—hence they can be omitted) and Curry has invented a formal system without variables. He found that the conditions on variables in substitution were so complicated (Church was the first to state them correctly, other authors got them wrong though their intuition enabled them to avoid mistakes in substitutions) that he invented a system without variables. One can do quite a lot with systems like these provided one does not want impredicative classes, but one likes to avoid these in any case. These two examples are mentioned only to show that the language of intelligent celestial beings might be as fantastic as their bodies might be, that is if they had anything that could be called bodies; they might be gaseous or a cloud of elementary particles related together in some way, more or less ghost-like creatures, (cf. Fred Hoyle's 'Black Cloud'.) Truth is stranger than fiction except in ghost stories.

It is for these reasons that I should limit the messages sent out to a fully formalised mathematics until we have got into contact and exhausted our mutual store of mathematical and scientific knowledge.

Anyway, if we do start sending messages into space (probably by satellites) we have all the time in the world, there is no hurry, so thousands of examples can be sent, hoping a receiver will perceive the general pattern we are trying to convey. And if it understands and replies in the same vein, then we can proceed. At a certain stage it might reply to the effect that it had an automatic recording machine so that we could speed up the messages and it could decode them at its leisure.

The text-programmes to be sent out would need careful scrutiny. There must be as few mistakes as possible, a misprint in a definition can be very puzzling. But the introduction of each new word by a new radio signal differing in wavelength or duration or both from any yet used, would be repeated in numerous text programmes with variations so that a mistake in one would be recognised as a mistake.

There are numerous misprints in spelling scattered through the book but all that I have found were easy to correct. There also are misprints in the text-programme in the main easy to correct.

As stated before, it seems easy to put over all mathematics and a great deal of science both A and B. For instance, once we know that they have computing machines, we could put over the equation of a function whose three-dimensional graph was the surface of an elephant. But it is difficult to see how some other disciplines are to be taught. Music might be out of the question for they might live on a celestial body without atmosphere. And how would we set about making them good Methodists or staunch Liberals?

I am not so happy about the chapter on behaviour. They might never understand Ha Inq Hb because they might have no language or even if they had they might be hermit-like creatures reproducing themselves after the manner of corms, and only using their language to supplement their memory. To make them understand Ha Inq Hb might be as futile as trying to make a blind man perceive colours. So the whole of the behaviouristic approach might be wasted, not so the formal mathematics.

But there are other objections on ethical grounds. There might be beings on some celestial body which have reached the stage when the meek inherit the earth and have completely forgotten their former hard life. Surely it would be a crime to tell them the whole of human behaviour as it is now and as it has been, or are we to hide the rapes, murders, robberies, wars, revolutions, deceits, frauds, etc., that make up a large part of human behaviour? Are we going to tell them about Tibet and things of that nature that make one ashamed to be human. Duke William's devastation of the North, the things Caesar did, the guillotine in the French revolution and so on, the cruelty of man to man. They might think they had contacted Hell itself, and it might give them ideas.

But these things could be hidden or wrapped up. But if human behaviour is to be sent over, then the subject of beliefs—which might be called the scourge of the human race, strong views cause wars, deductions from hypotheses are innocent—must be included. Beliefs do not appear in this volume but may in the next. But we could omit to mention unpleasant things and only mention good things. But then that is not telling them all of human behaviour. But what about playing games for money and gambling? In this volume this forms a gentle introduction to probability following the history of probability in this world. We are greatly indebted to the knights of the green table who asked the eminent mathematicians of their time to help them with their stakes. Another world might have a highly advanced theory of probability but they might never have thought of gambling, their whole monetary development might have been fantastically different from ours, or non-existent, so we might teach them to gamble and spoil their happiness.

Mathematics is pure, no-one fights about Goldbach's conjecture or other mathematical topics, though there has been an unfortunate argument about priority. So I think it better to stick to formalised mathematics until we are surer of the kind of being we are communicating with. We are likely to get more than we could ever hope this way.

Lastly this book should be of interest to philologists and linguists. It would introduce them to the syntax of formalised languages where the rules of formation and procedure and everything else is constructive and clear. No book on the grammar of any conventional language has yet been written or can be written with the clarity and finality of an account of the grammar of a formal language.

S. W. P. Steen

The usual difficulty occurs to the reader—that there is an unbridgeable gap between de Finetti's subjectivist theory and the objective components in the concept of probability as it is used in mathematics. On some interesting technical points, particularly that of the use of the concept of *exchangeability* instead of independence as a condition for getting a binomial distribution of the appropriate kind, the paper is interesting. However, the following sentence indicates that Braithwaite's own position is by no means that of a frequency theorist's in the usual sense: ' My answer, as a frequentist, is that, for the mass phenomena for which a frequency analysis holds, . . .' That is, Braithwaite does not think that the frequency analysis of probability is a logical analysis of the concept.

Now the second paper in this symposium, by Ayer, is of a quite different kind. It is in my view, without any question the most powerful attack on the conception of probability as a logical relation in the literature. It is highly organised, it moves on from point to point, meeting the objections of a logical relation theorist at every stage and raising still further difficulties, and yet within six pages it has provided a magnificently clear and, as far as one can see, absolutely devastating treatment of the problems associated with the ' requirement of total evidence '.

Popper's paper in a later symposium on what he calls ' the propensity interpretation of probability ' was equally interesting. He attempted to provide an objective conception of probability as a dispositional property of an experimental arrangement, and there seems no doubt from the discussion that ensued, and subsequent reflection, that this is greatly superior to the idea of probability as a property of a set of events in the sense which this is taken to be true by the frequency theorists, and equally superior to the subjective interpretations of de Finetti and others. Altogether, a student could probably learn more about the philosophical foundations of probability from reading these papers and the discussion thereafter than from anything else in the literature.

The papers on the role of philosophy in physics and on objectivity call for less comment. Körner's has already been reprinted, a tribute to its interest, and the substance of Polanyi's has appeared elsewhere. There was in general rather less philosophical meat in these, though possibly more philosophical interest for the physicists present. The last three philosophical papers were given at one session, and for the first time there was a noticeable deficiency in the organisation of the subsequent discussion. Only the first two of the papers were discussed explicitly, for reasons of time. These were the papers by Kneale and Gallie. The Kneale paper is an investigation of the ordinary linguistic usage of the word ' see '. It is interesting that this should come from a philosopher who is not thought of as primarily a linguistic analyst, and it was particularly interesting to see him and others defend the attacks of those who thought that this was mere etymological doodling, of no significance for physics or for philosophy of science. Both the investigation and the answering of these charges was extremely well done. Gallie's paper, about the possibility of predicting (*a*) new discoveries and (*b*) the behaviour of machines that are trying to predict the predictor's behaviour (*à la* Popper), was also very stimulating and led to a fruitful discussion. The final paper, by Ryle, is one of his usual lucid and persuasive arguments-in-a-nutshell. It is an important contribution to the understanding of the problem of induction, and contains remarks as a preface which completely undermine various naïve theories of prediction, such as those which confuse it with predictive argument. It is unfortunate that it was not discussed.

All in all the book provides us with more philosophical entertainment, along with philosophical content, than one comes across in a very long time.

MICHAEL SCRIVEN

A History of Ancient Geography. By E. H. Bunbury. Second Edition. Dover Publications Inc., New York, 1959. Vol. I, pp. xxxiv + 666. Vol. II, pp. xviii + 743. $12.50 each.

BUNBURY's massive work was first published in 1879; a second edition (the one now republished) appeared in 1883. To the modern reader it epitomises the achievements and the limitations of High Victorian classical scholarship. For Bunbury's specific type of intelligence, his mode of scholarship, are now as archaic and as alien to us as the rolling periods of his classical prose.

He wrote at the very end of an era in the development of geography. In that era, men who called themselves geographers, both in England and France, were concerned chiefly to establish the elementary facts of the location of the features of the earth's surface. The process had been a long and hard one. It had started in the thirteenth century with the first significant developments in the making of accurate mariners' charts; had continued with the great age of discovery and the rapid advances in navigational instruments, in the fifteenth and sixteenth centuries; and had culminated in the introduction of accurate surveys for detailed large-scale topographic maps, in the early nineteenth century. In 1879, the last remote spots of interior Africa were just being fixed on the world map[1]; and triangulation survey was being applied rapidly to the populous parts of the earth's surface. Geography's apprenticeship, therefore, was over.

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[1] Bunbury could still quote points in Africa which could not be fixed accurately by 'modern geographers': vol. II, p. 553 n.

Coming when it did, Bunbury's work reflected the preoccupations of the age that was closing, and serves as an epitaph upon it. In that period, it was only natural that geographers should interest themselves in the Classical era. For the objectives of the Victorian geographers were also those of the geographers of antiquity: to produce an ordered account of the physiognomy of the earth. This object Bunbury refers to by the now-archaic term 'positive' geography,[1] of which a most important part was physical geography, that is the delineation of the leading physical features of the continents of the world. Positive geography must in turn be based on accurate maps produced by scientific measurement ('technical'[2] or 'scientific'[3] geography). For the Ancients, Bunbury concluded, scientific geography was impossible: the devices necessary for accurate measurement of distance, (astronomical for latitude, chronological for longitude) remained unsolved because the classical world failed to apply its abstract science technologically[4], so that as late as Ptolemy (fl. A.D. 150–160) even Italy was not accurately delineated.[5] While for areas remote from Europe, the process of exploration had not even accumulated approximate knowledge: Ptolemy was vague and inaccurate on eastern Asia, completely ignorant of southern Africa and the New World.[6] In the critical foundations of the subject, therefore, the Victorians had by 1879 just succeeded where the Ancients had failed.

Foundations, however, are not very interesting; we require only that they bear the weight of the building which obscures them. It was in the thirty years after Bunbury's work that the creation of modern geography took place. In England, geography was established as a university subject, with the creation of a Readership at Oxford in the late eighties, and of a School of Geography there in 1899; in this School, the study of Regional Geography was taken up by Mackinder and Herbertson; and the modern science of geomorphology was born, with the publication of the two great papers by the American W. M. Davis in the newly-founded *Journal of the Geographical Society of London.*[7] In the years 1880–1914 a coherent academic discipline emerged, based on the methodological works of German writers like von Humboldt, Ritter, and Hettner. The new methodology was based on the concept of Ritter that:

> Die geographische Wissenschaften haben es vorzugsweise mit den Räumen der Erdoberfläche zu tun, in so fern diese irdisch (sei es auch immer welchen, Naturreichen angehörig, und mit welchen Formen ausgestattet) erfüllt sind;

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[1] Bunbury Vol. I, p. 401; Vol. II, p. 230

[2] Ibid. Vol. II, p. 34

[3] Ibid. Vol. I, pp. 120–124, 395, 615

[4] Ibid. Vol. II, p. 553

[5] Ibid. Vol. II, pp. 594–595

[6] Ibid. Vol. II, pp. 604–608, 633–634

[7] 'The Development of Certain English Rivers', *Geographical Journal*, 1895, **5**, 127–146; 'The Geographical Cycle', Ibid. 1899, **14**, 481–504.

> also mit den Beschreibung und Verhältnissen des Nebeneinander der Örtlichkeiten, als solche, in ihren besonderen Vorkommen wie in ihren allgemeinsten tellurischen Erscheinungen.[1]

In other words: the study not merely of areas, not merely of phenomena, but of the peculiar character given to areas by the phenomena within them and by their relation each with the other and with the area. In this view, geography is the scientific study of a real differentiation of the world. It studies the ' data that contribute significantly, both in themselves and in their causal relations to other variables, to the total complex of areal differentiation '.[2] Its centre is the study of regional geography (*Länderkunde*).

Of these developments hardly a premonition appears in Bunbury's pages. A ' scientific comprehension ' of the subject, for Bunbury as for the classical geographers, consisted in the ' geographical outlines of the general structure and composition of the continents ' as opposed to ' mere dry catalogues of the names of cities, or tribes, rivers and mountains '.[3] Once or twice only, in all these fourteen hundred pages, does a vision of modern geography appear as a science concerned with the exact measurement of the variation of phenomena over the earth's surface, and the relations between these variations. And these come, ironically, not from men calling themselves geographers, but from philosophers: Pythagoras and Aristotle.[4] Here are found first approaches to modern geomorphological ideas and climatic classification. But, as Bunbury is quick to point out, Aristotle's ' positive geography ' was sadly lacking.[5]

P. G. Hall

The Language of Mathematics. By F. W. Land.
John Murray, London, 1960. Pp, 264. 21s.
Mathematics in the Making. By Lancelot Hogben.
Macdonald, London, 1960. Pp. 320. 50s.

F. W. Land does not coat his mathematical pills with saccharine, nor does he resort to placebos of the mathematically quaint and curious. He has set himself the task of making common mathematical concepts and operations comprehensible and attractive, and he succeeds. Wisely avoiding the calculus, he takes the reader through numbers, units, algebra, graphs, logarithms, up to the normal distribution in statistics. Nowhere does the reader find those indigestible chunks of symbolic argument coming at the crucial point of an informal discussion. Everywhere there are neat two-colour graphs and diagrams illustrating the text. One section I would recommend even for research workers in the exact sciences is the discussion of decimal fractions, and the meaning of 'significant figures'. There are numerous references to common objects and popular science, not displacing but enriching the mathematical argument; and where the author has a flight of fancy, with Fibonacci numbers, he does so with elegance. This book seems to me to be the answer to many a teacher's prayers, and would be most useful for anyone wanting to recall half-forgotten school mathematics. At a guinea, it is extremely good value just as a piece of book; the publishers as well as the author should be congratulated on their work.

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[1] C. Ritter, *Einleitung zur allegemeinen vergleichenden Geographie*, Berlin, 1852, p. 152
[2] R. Hartshorne, *The Nature of Geography*, Lancaster, Pa., 1939, p. xi
[3] Bunbury, op. cit. Vol. II, p. 374
[4] Ibid. Vol. I, pp. 125, 397-398
[5] Ibid. Vol. I, pp. 399-401

How times change! Back in the innocent 'thirties (when, in the words of the *New Yorker* cartoon, all we had to worry about was where our next meal was coming from) Lancelot Hogben amused himself in hospital by writing his *Mathematics for the Million*, and was astonished when first a publisher and then the public liked it. Now the amateur is displaced by Team Research, and Hogben has directed the production of a glossy lithographed volume, full of multi-colour diagrams and the most enchanting plates. The purpose remains the same as in the earlier book: to help in the understanding of mathematics through an acquaintance with its history.

Hogben knows quite well what he is and is not doing, so it would be pointless to criticise the book as a self-contained essay in the social history of mathematics. The work is sufficiently well done, within its own terms of reference, for the question of its programme to be considered seriously. In brief, what can the history of mathematics teach us about mathematics? First, it can teach us that mathematics has a history, that each theorem d technique was invented by someone. This helps to dispel the impression gained when studying mathematics at any but the most advanced level, that it is an impersonal and eternal monolith of abstract truths. Moreover, by showing that the mathematics pursued at any period was in some fashion related to the applications of mathematics, Hogben may help to break down the distinction between 'basic' mathematics (what the schoolmaster learned when young) and 'frills' (introduction to modern techniques, such as computers).

But there remains the question of how much use is the historical approach in the comprehension of modern elementary mathematics. Hogben knows the problem here, but I am not sure that he realises its implications. The course of history is tortuous; the calculus was not invented in neat stages following the chapters of a modern text. Many techniques, such as logarithms, were so swaddled in outworn concepts and special techniques by their inventors, that the form they first took is entirely unsuitable for teaching. Hogben does not lose sleep over this, any more than he has patience for (or interest in) the difficulties in the concept of infinity. Given a topic, he sketches the social-historical background, and then sails into an abbreviated sketch of the modern elementary theory. I found this (to my dismay) when I first read *Mathematics for the Million*; each chapter was neatly divided into two, one half fascinating, the other incomprehensible. The division persists in the present book; the uniform scattering of illustrations does not mitigate it.

Hence the title of the book is misleading, and the aim of ' bringing a new humanistic approach ' succeeds only partially. The brute fact is that mathematics, like any other discipline, physics, history, or playing the piano, requires hard, concentrated work for mastery at any level. To ignore this fact, and to attempt to ' bridge gaps ' by historical talking-around a subject, is to achieve little more than sugar-coating the pill.

J. Ravetz

The Growth of Scientific Physiology. By G. J. Goodfield.
Hutchinson, London, 1960. Pp. 174. 18s.

This book is said to have three main strands. The first tells the story of the gradual emergence of scientific physiology ' with its own concepts, methods of enquiry and types of explanation '. The second strand consists of an account of the philosophical debates which arose concerning physiological methodology—the vitalist-mechanist controversy. Should physico-chemical concepts appear in physiological explanations? Lastly, there is an account of the ' progressive clarification of men's ideas about the origin and control of animal heat '. I take it that this ordering which Miss Goodfield has given to her strands indicates the ordering of her aims in writing the book. In practice the book reads as an account of the history of the concept of animal heat. More than half the book is devoted to an interesting and well documented account of this subject up to the time of Claude Bernard. It is a pity that the title did not reflect this part of the book.

The author used the topic of the history of the concept of animal heat to illustrate her other two points, and from the philosophy of science aspect we can judge the book on this. Both topics are interwoven in chapters which bring us methodologically up to date at various periods. In principle this should be a useful way of presenting the history of ideas. However, the result is unsatisfactory. This is because Miss Goodfield does not make explicit what she means by a scientific physiology. Except in a final discussion of Claude Bernard's concept of an internal environment we do not see any example of a purely physiological concept. Neither do we see what *is* a physiological method of inquiry or a physiological type of explanation. We watch the introduction of chemistry and the physical sciences into explanations of physiological phenomena and are left with what Woodger called the ontological vitalists saying that this will never be sufficient. We see the explanatory vitalists (Miss Goodfield's term) arise. But in the end Bernard appears as a mechanist opposed to Liebig who needed a vital force in his explanatory concepts. What we do not see is why one is more scientific than the other. This should have been made explicit and I think could only be done by the author clearly stating what she conceives to be the structure of physiological theories. Thought of as a set of explanatory hypotheses covering physiological observations it is obvious that if one makes chemical observations one explains in terms of chemical explanations. Therefore we want to be able to see what would be a purely physiological observation. Incidentally if we could see a set of statements arranged in explanatory levels, whereabouts would Claude Bernard's concept of the constancy of internal environment come? Is it a generalisation of what we see? Is it explanatory? Is it possibly a methodological postulate, and is this why it was important? These are questions that have relevance to the author's theme and which should have been answered.

Finally one can question the choice of animal heat as the idea whose history demonstrates the rise of scientific physiology. Little is said about the importance of measurement in physiology, and nothing is said about the importance of certain types of measuring apparatus. For example, in so far as for many years it was almost true to say that no paper would be published in a physiological journal unless it reported observations made with a kymograph, this would seem to indicate the importance which physiologists attach to this piece of apparatus in so far as a scientific physiology is concerned. Yet no mention is made of Carl Ludwig who invented the kymograph, and whose pupils have had a far greater influence on modern physiology than Claude Bernard.

R. F. J. Withers

Théorie synthétique de la rélativité restreinte et des quanta. By O. Costa de Beauregard.
Gauthiers-Villars, Paris, 1957. Pp. xii + 200 pp. $9.30.

It is difficult to see for what audience this book is intended. It supposes a familiarity (including mathematical detail) with special relativity, Hilbert space, matrices and operators, and elementary quantum mechanics (including spin); so it cannot be said to be popular. But it does not pretend to be a complete text on quantum field theory, for the treatment of infinities, renormalisation, the consistency of quantum electrodynamics, the Lamb shift and the anomalous magnetic moment of the electron are left on one side. There is, then, a danger that the book may fall between two stools, and this would be a pity, for what it does it does well. It is an account of those investigations of quantum field theory that throw most light on the connection between special relativity and quantum theory.

About half the book is an account of such relatively elementary topics as the four-dimensional form of the uncertainty relations, and the wave-mechanics of a particle (with or without spin) in a specified field. This half concludes with a discussion of von Neumann's demonstration of the impossibility of hidden variables in quantum mechanics. The second half begins with a simple description of the Tomonaga-Schwinger quantum field theory, and of Dyson's derivation of Feynman's rules. Then the classical Wheeler-Feynman electrodynamics is presented, and the last chapter discusses the perennial question of the contrast between microscopic reversibility and macroscopic irreversibility.

The treatment is not too technical; but in such a small space it is also clearly impossible to say much about the philosophical questions that arise. The book can be recommended to anyone, with most of the mathematical equipment listed, who wishes to have a general view of quantum field theory; so long as they remember that further investigations of the questions that have been omitted from the book may well lead to radical revisions of the basic parts that have been included.

C. W. KILMISTER

Founders of British Science. By J. G. Crowther.
The Cresset Press, London, 1960. Pp. 296. 35s.

THIS book is one of a number written to mark the tercentenary of the foundation of the Royal Society. In it Mr Crowther traces the life and work of six of the most eminent men among its early members: Wilkins, Boyle, Ray, Wren, Hooke, and Newton. All these men did, in different ways, make important contributions to the development of science in Britain at the end of the seventeenth century and it was thus an excellent idea of Mr Crowther to collect together in one volume a great deal of interesting information about them.

Unfortunately the book has some bad faults. The most obvious is the style which is unbearably clumsy and monotonous. The division into paragraphs follows no coherent principles; sentences follow each other in quick staccato succession; commas are scattered liberally and inaccurately whereas semi-colons are seldom if ever employed. And so although the book is clearly meant for the general reader it is almost impossible to read it as a consecutive narrative.

Almost as serious a defect—or perhaps one should say another aspect of the same defect—is the way in which the chapters are arranged. There are no subdivisions whatsoever so that one finds sudden jumps as the author moves without warning from one topic to the next: sometimes biographical anecdotes, sometimes reflections on the relation of science and politics, sometimes digressions into the general history of the period—digressions which are often misleading as, for example, the statement that Laud helped Strafford to extort money from the great Earl of Cork by accusing him of religious offences. This absence of structure makes it difficult for the reader to grasp the different strands in the narrative and at the same time renders the book almost useless for reference purposes.

Finally the book is marred by the author's unquestioning acceptance of the Marxist interpretation of history. We are told on the first page that until the discovery of America in 1492 scientific activities in Britain were 'a part of the European feudal monastic culture'. Later we read that 'broadly speaking, the development of science (*sc.* in Britain after 1660) was to be directed along those channels which were to be most useful in the forthcoming period of imperial and capitalist expansion'. The overworked example of the longitude prize is brought in on every possible occasion. And then after quoting Newton's claim that gravity 'abundantly serves to account for all the motions of the celestial bodies, and of our sea' Crowther comments, 'This last phrase was peculiarly British, for at the beginning of the period of Britain's maritime supremacy, the sea was almost as important to the British as the rest of the universe.' This may be so but it is worth remembering that Newton had spent a great deal of time in working out his theory of the tides and that this was a problem which had defeated all previous scientists including Galileo.

H. G. Alexander

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